EXPERIMENTAL ENQUIRY

CONCERNING THE

Natural Powers of Wind and Water

TO TURN

MILLS AND OTHER MACHINES

DEPENDING ON A

CIRCULAR MOTION.

AND AN

EXPERIMENTAL EXAMINATION

OF THE

QUANTITY AND PROPORTION

Of MECHANIC POWER

Neceſſary to be employed in giving different degrees of VELOCITY to HEAVY BODIES from a STATE of REST.

ALSO

NEW FUNDAMENTAL EXPERIMENTS

UPON THE

COLLISION OF BODIES.

WITH FIVE PLATES OF MACHINES.

By THE LATE MR. JOHN SMEATON, F. R. S.

THE SECOND EDITION.

LONDON:
PRINTED FOR I. AND J. TAYLOR,
NO. 56, HIGH-HOLBORN.

1796.

ADVERTISEMENT.

THE subjects of the following sheets, as branches of practical mechanics, are interesting; and the manner in which they are treated render them important.

Previous to these experiments, the superior power of over-shot water-wheels was not only doubted, but some authors of considerable celebrity had given a preference to undershot wheels.---The experiments made, and here fully explained by Mr. SMEATON *(who, as a practical mechanic, must ever be rated in the first class), shew the fallacy of former conclusions, and the great superior power of overshot wheels: the lapse of time since the publication of these principles, has confirmed their accuracy, and has established a durable reputation for their author.*

These Essays, published originally in the Philosophical Transactions (the parts of which are now become scarce, and of high price on their account), are now offered to the public at a moderate purchase; to render them accessible to every

every ingenious enquirer, must tend to spread the very useful information they contain. To a superior mind nothing can be more grateful, than imparting to others the advantages of that superiority, which bountiful Heaven has bestowed on enlightened Genius.

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AN EXPERIMENTAL ENQUIRY CONCERNING THE NATURAL POWERS OF WATER AND WIND

To turn Mills and other Machines depending on A CIRCULAR MOTION:

Read before the Royal Society, May 3 and 10, 1759.

WHAT I have to communicate on this ſubject was originally deduced from experiments made on working models, which I look upon as the beſt means of obtaining the outlines in mechanical enquiries. But in this caſe it is very neceſſary to diſtinguiſh the circumſtances in which a model differs from a machine in large; otherwiſe a model is more apt to lead us from the truth than towards it. Hence the common obſervation, that a thing may do very well in a model that will not anſwer in large. And, indeed, though the utmoſt circumſpection be uſed in this way, the beſt ſtructure of machines cannot be fully aſcertained, but by making trials with them, when made of their proper ſize. It is for this reaſon that though the models re-

ferred

ferred to, and the greateſt part of the following experiments, were made in the years 1752 and 1753, yet I deferred offering them to the Society, until I had an opportunity of putting the deductions made therefrom in real practice, in a variety of caſes, and for various purpoſes; ſo as to be able to aſſure the Society that I have found them to anſwer.

PART I.

Concerning Underſhot Water-Wheels:

PLATE I. Fig. 1. is a perſpective view of the machine for experiments on water-wheels; wherein

ABCD is the lower ciſtern, or magazine, for receiving the water, after it has quitted the wheel; and for ſupplying

D E the upper ciſtern, or head; wherein the water being raiſed to any height required, by a pump, that height is ſhewn by

F G, a ſmall rod, divided into inches and parts; with a float at the bottom, to move the rod up and down, as the ſurface of the water riſes and falls.

H I is a rod by which the ſluice is drawn, and ſtopt at any height required, by means of

K a pin, or peg, which fits ſeveral holes, placed in the manner of a diagonal ſcale, upon the face of the rod H I.

G L is the upper part of the rod of the pump, for drawing the water out of the lower ciſtern, in order to raiſe and keep

keep up the ſurface thereof at its deſired height, in the head D E; thereby to ſupply the water expended by the aperture of the ſluice.

M M is the arch and handle for working the pump, which is limited in its ſtroke by

N, a piece for ſtopping the handle from raiſing the piſton too high; that alſo being prevented from going too low, by meeting the bottom of the barrel.

O is the cylinder, upon which a cord winds, and which, being conducted over the pullies P and Q, raiſes

R, the ſcale, into which the weights are put, for trying the power of the water.

S T, the two ſtandards, which ſupport the wheel, are made to ſlide up and down, in order to adjuſt the wheel, as near as poſſible, to the floor of the conduit.

W the beam which ſupports the ſcale and pullies; this is repreſented as but little higher than the machine, for the ſake of bringing the figure into a moderate compaſs, but in reality is placed 15 or 16 feet higher than the wheel.

Plate II. Fig. 2. is a ſection of the ſame machine, wherein the ſame parts are marked with the ſame letters as in Fig. 1. Beſides which

X X is the pump barrel, being 5 inches diameter, and 11 inches long.

Y is the piſton; and

Z the fixed valve.

G V is a cylinder of wood, fixed upon the pump-rod, and reaches above the ſurface of the water: this piece of wood being of ſuch a thickneſs, that its ſection is half the area of that of the pump-barrel, will cauſe the ſurface of water to riſe in the head, as much while the piſton is deſcending, as while it is riſing: and will thereby keep the gauge-rod F G more equally to its height.—*Note*, The arch and handle M M is here repreſented on a different ſide to what it is ſhewn in the preceding figures, in order that its dimenſions may the better appear.

a a ſhews one of the two wires which ſerve as directors to the float, in order that the gauge-rod F G may be kept perpendicular; for the ſame purpoſe alſo ſerve *w*, a piece of wood with a hole to receive the gauge-rod, and keep it upright.

b is the aperture of the ſluice.

c c a kant-board, for throwing the water more directly down the opening *c d*, into the lower ciſtern; and

c e is a ſloping board, for bringing back the water that is thrown up by the floats of the wheel.

Fig. 3. repreſents one end of the main axis, with a ſection of the moveable cylinder, marked O in the preceding figures.

A B C D is the end of the axis; whereof the parts

B and D are covered with ferrules or hoops of braſs.

E is a cylinder of metal; whereof the part marked

F is the pivot or gudgeon.

c c is

c c is the fection of an hollow cylinder of wood, the diameter of the interior part being fomewhat larger than the cylindrical ferrule B.

a a is the fection of a ferrule of brafs, driven into the end of the hollow cylinder, and which is adjufted to that marked B, fo as to flide freely thereupon, but with as little fhake as poffible.

b b, *d d*, *g g*, reprefent the fection of a brafs ferrule, plate, and focket, fixed upon the other end of the hollow cylinder; the focket *d d* being adjufted to flide freely upon the cylinder E, in the fame manner as the ferrule *a a* flides upon the cylinder B: the outer end of the focket at

g g is formed into a fort of button; by pufhing whereof, the hollow cylinder will move backwards and forwards, or turn round at pleafure upon the cylindrical parts of the axis B and E.

e e, *i i*, *o o*, reprefent the fection of a brafs ferrule, alfo fixed upon the hollow cylinder: the edge of this ferrule

e e is cut into teeth, in the manner of a *contrate* wheel; and the edge thereof

o o is cut in the manner of a ratchet.

Of confequence, when the plate *b d d b* is pufhed clofe to the ferrule D, the teeth of the ferrule *e e* will lay hold of

G, a pin fixed into the axis; by which means the hollow cylinder is made to turn along with the wheel and axis: but being drawn back by the button *g g*, the hollow cylinder is thereby difengaged from the pin G, and ceafes

 turning.

turning. *Note*, the weight in the ſcale is prevented from running back, by a catch that plays in and lays hold of the ratchet *o o*.

By this means the hollow cylinder upon which the cord winds and raiſes the weight, is put in action and diſcharged therefrom inſtantaneouſly, while the wheel is in motion: for, without ſome contrivance of this kind, it would not be eaſy to make this ſort of experiments with any tolerable degree of exactneſs.

The uſe of the apparatus now deſcribed will be rendered more intelligible, by giving a general idea of what I had in view; but as I ſhall be obliged to make uſe of a term which has heretofore been the cauſe of diſputation, I think it neceſſary to aſſign the ſenſe in which I would be underſtood to uſe it; and in which I apprehend it is uſed by practical *Mechanicks*.

The word *Power*, as uſed in practical mechanicks, I apprehend to ſignify the exertion of ſtrength, gravitation, impulſe, or preſſure, ſo as to produce motion: and by means of ſtrength, gravitation, impulſe, or preſſure, compounded with motion, to be capable of producing an effect: and that no effect is properly mechanical, but what requires ſuch a kind of power to produce it.

The raiſing of a weight, relative to the height to which it can be raiſed in a given time, is the moſt proper meaſure of power; or, in other words, if the weight raiſed is multiplied by the height to which it can be raiſed in a given time, the product is the meaſure of the power raiſing it; and conſequently, all thoſe powers are equal, whoſe products, made by ſuch multiplication, are equal: for if a power can raiſe twice the weight to the ſame height; or the ſame weight to twice the height, in the ſame time that another power can, the firſt power is double the ſecond;

ſecond: and if a power can raiſe half the weight to double the height; or double the weight to half the height, in the ſame time that another can, thoſe two powers are equal. But *note*, all this is to be underſtood in caſe of ſlow or equable motion of the body raiſed; for in quick, accelerated, or retarded motions, the *vis inertiæ* of the matter moved will make a variation.

In comparing the effects produced by water-wheels with the powers producing them; or, in other words, to know what part of the original power is neceſſarily loſt in the application, we muſt previouſly know how much of the power is ſpent in overcoming the friction of the machinery, and the reſiſtance of the air; alſo what is the real velocity of the water at the inſtant that it ſtrikes the wheel; and the real quantity of water expended in a given time.

From the velocity of the water, at the inſtant that it ſtrikes the wheel, given, the height of head productive of ſuch velocity can be deduced, from acknowledged and experimented principles of hydroſtatics: ſo that by multiplying the quantity, or weight of water, really expended in a given time, by the height of a head ſo obtained; which muſt be conſidered as the height from which that weight of water had deſcended in that given time; we ſhall have a product, equal to the original power of the water, and clear of all uncertainty that would ariſe from the friction of the water, in paſſing ſmall apertures; and from all doubts, ariſing from the different meaſure of ſpouting waters, aſſigned by different authors. On the other hand, the ſum of the weights raiſed by the action of this water, and of the weight required to overcome the friction and reſiſtance of the machine, multiplied by the height to which the weight can be raiſed in the time given, the product will be equal to the effect of that power; and the proportion of the two products will be the proportion of the *power* to the *effect*: ſo that by loading the wheel with different weights ſucceſſively, we ſhall

be able to determine at what particular load, and velocity of the wheel, the effect is a *maximum*.

The manner of finding the real velocity of the water, at the instant of its striking the wheel; the manner of finding the value of the friction, resistance, &c. in any given case; and the manner of finding the real expence of water, so far as concerns the following experiments, without having recourse to theory; being matters upon which the following determinations depend, it will be necessary to explain them.

To determine the Velocity of the Water striking the Wheel.

It has already been mentioned, in the references to the figures, that weights are raised by a cord winding round a cylindrical part of the axis. First, then, let the wheel be put in motion by the water, but without any weights in the scale; and let the number of turns in a minute be 60: now it is evident, that was the wheel free from friction and resistance, that 60 times the circumference of the wheel would be the space through which the water would have moved in a minute: with that velocity wherewith it struck the wheel: but the wheel being encumbered by friction and resistance, and yet moving 60 turns in a minute, it is plain that the velocity of the water must have been greater than 60 circumferences before it met with the wheel. Let now the cord be wound round the cylinder, but contrary to the usual way, and put a weight in the scale; the weight so disposed (which may be called the *counter-weight*) will endeavour to assist the wheel in turning the same way, as it would have been turned by the water: put therefore as much weight into the scale as, without any water, will cause it to turn somewhat faster than at the rate of 60 turns in a minute; suppose 63; let it now be tried again by the water, assisted by the weight; the wheel there-

therefore will now make more than 60 turns; ſuppoſe 64: hence we conclude the water ſtill exerts ſome power in giving motion to the wheel. Let the weight be again increaſed, ſo as to make $64\frac{1}{2}$ turns in a minute without water: let it once more be tried with water as before; and ſuppoſe it now to make the ſame number of turns with water as without, *viz.* $64\frac{1}{2}$: hence it is evident, that in this caſe the wheel makes the ſame number of turns in a minute, as it would do if the wheel had no friction or reſiſtance at all; becauſe the weight is equivalent thereto; for was it too little the water would accelerate the wheel beyond the weight: and if too great, retard it; ſo that the water now becomes a *regulator* of the wheel's motion; and the velocity of its circumference becomes a meaſure of the velocity of the water.

In like manner, in ſeeking the greateſt product, or *maximum*, of effect; having found by trials what weight gives the greateſt product, by ſimply multiplying the weight in the ſcale by the number of turns of the wheel, find what weight in the ſcale, when the cord is on the contrary ſide of the cylinder, will cauſe the wheel to make the ſame number of turns the ſame way, without water; it is evident that this weight will be nearly equal to all friction and reſiſtance taken together; and conſequently, that the weight *in* the ſcale, with twice * the weight *of* the ſcale, added to the back or counter-weight, will be equal to the weight that could have been raiſed, ſuppoſing the machine had been without friction or reſiſtance; and which multiplied by the height to which it was raiſed, the product will be the greateſt effect of that power.

* The weight of the ſcale makes part of the weight both ways.

The

The Quantity of Water expended is found thus:

The pump made ufe of for replenifhing the head with water was fo carefully made, that, no water efcaping back by the leathers, it delivered the fame quantity of water at every ftroke, whether worked quick or flow; and as the length of the ftroke was limited, confequently the value of one ftroke (or, on account of more exactnefs, 12 ftrokes) was known, by the height to which the water was thereby raifed in the head; which, being of a regular figure, was eafily meafured. The fluice, by which the water was drawn upon the wheel, was made to ftop at certain heights by a peg; fo that when the peg was in the fame hole, the aperture for the effluent water was the fame. Hence the quantity of water expended by any given head, and opening of the fluice, may be obtained: for, by obferving how many ftrokes a minute was fufficient to keep up the furface of the water at the given height, and multiplying the number of ftrokes by the value of each, the water expended by any given aperture and head in a given time will be given.

Thefe things will be further illuftrated by going over the *calculus* of one fet of experiments.

Specimen of a Set of Experiments.

The fluice drawn to the firft hole.

The water above the floor of the fluice	30	Inches.
Strokes of the pump in a minute —	$39\frac{1}{2}$	
The head raifed by 12 ftrokes —	21	Inches.

The wheel raifed the empty fcale, and made turns in a minute	80
With a counter-weight of 1 lb. 8 oz. it made - -	85
Ditto tried with water - - - - -	86

No.

No.		Weight. lb.	oz.		Tuns in a Min.		Product.
1	—	4	0	—	45	—	180
2	—	5	0	—	42	—	210
3	—	6	0	—	$36\frac{1}{4}$	—	$217\frac{1}{2}$
4	—	7	0	—	$33\frac{3}{4}$	—	$236\frac{1}{4}$
5	—	8	0	—	30	—	240 maximum.
6	—	9	0	—	$26\frac{1}{2}$	—	$238\frac{1}{2}$
7	—	10	0	—	22	—	220
8	—	11	0	—	$16\frac{1}{2}$	—	$181\frac{1}{2}$
9	—	12	* ceafed working.				

Counter-weight, for 30 turns without water, 2 oz. in the fcale.

N. B. The area of the head was 105,8 fquare inches.
Weight of the empty fcale and pulley, 10 oz.
Circumference of the cylinder, 9 inches.
Circumference of the water-wheel, 75 ditto.

Reduction of the above Set of Experiments.

The circumference of the wheel, 75 inches, multiplied by 86 turns, give 6450 inches for the velocity of the water in a minute; $\frac{1}{60}$ of which will be the velocity in a fecond, equal to 107,5 inches, or 8,96 feet, which is due to a head of 15 inches †; and this we call the *virtual* or *effective* head.

The

* *N. B.* When the wheel moves fo flow as not to rid the water fo faft as fupplied by the fluice, the accumulated water falls back upon the aperture, and the wheel immediately ceafes moving.

† This is determined upon the common maxim of hydroftatics, that the velocity of fpouting waters is equal to the velocity that an heavy

The area of the head being 105,8 inches, this multiplied by the weight of water of the inch cubic, equal to the decimal ,579 of the ounce avoirdupoise, gives 61,26 ounces for the weight of as much water, as is contained in the head, upon 1 inch in depth, $\frac{1}{16}$ of which is 3,83 pounds; this multiplied by the depth 21 inches, gives 80,43 lb. for the value of 12 strokes; and by proportion, $39\frac{1}{2}$ (the number made in a minute) will give 264,7 lb. the weight of water expended in a minute.

Now as 264,7 lb. of water may be considered as having descended through a space of 15 inches in a minute, the product of these two numbers 3970 will express the *power* of the water to produce mechanical effects; which were as follows:

The velocity of the wheel at the *maximum*, as appears above, was 30 turns a minute; which multiplied by 9 inches, the circumference of the cylinder, makes 270 inches; but as the scale was hung by a pulley and double line, the weight was only raised half of this, *viz.* 135 inches.

The weight in the scale at the maximum	8 *lb.*	0 *oz.*
Weight of the scale and pulley -	0	10
Counter-weight, scale, and pulley -	0	12
Sum of the resistance - -	9	6
	or *lb.* 9,375.	

Now as 9,375 lb. is raised 135 inches, these two numbers being multiplied together, the product is 1266, which expresses the effect produced at a maximum: so that the proportion of the *power* to the *effect* is as 3970 : 1266, or as 10 : 3,18.

heavy body would acquire in falling from the height of the reservoir; and is proved by the rising of jets to the height of their reservoirs nearly.

But

But though this is the greateſt *ſingle* effect producible from the power mentioned, by the impulſe of the water upon an underſhot wheel; yet, as the whole power of the water is not exhauſted thereby, this will not be the true ratio between the *power* of the water, and the *ſum* of all the *effects* producible therefrom: for as the water muſt neceſſarily leave the wheel with a velocity equal to the wheel's circumference, it is plain that ſome part of the power of the water muſt remain after quitting the wheel.

The velocity of the wheel at the maximum is 30 turns a minute; and conſequently its circumference moves at the rate of 3,123 feet a ſecond, which anſwers to a head 1,82 inches; this being multiplied by the expence of water in a minute, *viz.* 264,7 lb. produces 481 for the power *remaining* in the water after it has paſſed the wheel: this being therefore deducted from the original power 3970, leaves 3489, which is that *part* of the power which is ſpent in producing the effect 1266; and conſequently the part of the power ſpent in producing the effect, is to the greateſt effect producible thereby as 3489 : 1266 : : 10 : 3,62, or as 11 to 4.

The *velocity of the water* ſtriking the wheel has been determined to be equal to 86 circumferences of the wheel per minute, and the *velocity of the wheel* at the *maximum* to be 30; the velocity of the water will therefore be to that of the wheel as 86 to 30; or as 10 to 3,5, or as 20 to 7.

The *load at the maximum* has been ſhown to be equal to 9 lb. 6. oz. and that the wheel ceaſed moving with 12 lb. in the ſcale: to which if the weight of the ſcale is added, *viz.* 10 ounces*, the proportion will be nearly as 3 to 4 between

* The reſiſtance of the air in this caſe ceaſes, and the friction is not added, as 12 lb. in the ſcale was ſufficient to ſtop the wheel after it had been in full motion; and therefore ſomewhat more than a counterbalance to the impulſe of the water.

the

the load at the *maximum* and *that* by which the wheel is ſtopped.

It is ſomewhat remarkable, that though the velocity of the wheel in relation to the water turns out greater than $\frac{1}{3}$ of the velocity of the water, yet the impulſe of the water in the caſe of a *maximum* is more than double of what is aſſigned by theory; that is, inſtead of $\frac{4}{9}$ of the column, it is nearly equal to the whole column.

It muſt be remembered, therefore, that in the preſent caſe, the wheel was not placed in an open river, where the natural current, after it has communicated its impulſe to the float, has room on all ſides to eſcape, as the theory ſuppoſes; but in a conduit, or race, to which the float being adapted, the water cannot otherwiſe eſcape than by moving along with the wheel. It is obſervable, that a wheel working in this manner, as ſoon as the water meets the float, receiving a ſudden check, it riſes up againſt the float, like a wave againſt a fixed object; inſomuch that when the ſheet of water is not a quarter of an inch thick before it meets the float, yet this ſheet will act upon the whole ſurface of a float, whoſe height is 3 inches; and conſequently was the float no higher than the thickneſs of the ſheet of water, as the theory alſo ſuppoſes, a great part of the force would have been loſt, by the water daſhing over the float*.

In

* Since the above was written, I find that Profeſſor Euler, in the Berlin Acts for the year 1748, in a memoir entitled *Maxims pour aranger le plus avantageuſement les machines deſtinees a elever de l'eau par le moyen de pompes,* page 192. § 9. has the following paſſage; which ſeems to be the more remarkable, as I do not find he has given any demonſtration of the principle therein contained, either from theory or experiment; or has made any uſe thereof in his calculations on this ſubject:——" Cependant dans ce cas puiſque " l'eau eſt reflechie, & qu'elle decoule ſur les aubes vers les cotés, " elle y exerce encore une force particuliere, dont l'effet de l'im- " pulſion

Ratio of the velocity of the water and wheel.	Ratio of the load at the equilibrium, to the load at the maximum.	Experiments
10:3,4	10:7,75	At the 1st hole.
10:3,5	10:7,4	
10:3,4	10:7,5	
10:3,55	10:7,53	
10:3,45	10:7,32	
10:3,36	10:8,02	
10:3,6	10:8,3	
10:3,77	10:9,1	
10:3,65	10:9,1	
10:3,8	10:9,3	
10:3,66	10:7,9	At the 2d.
10:3,62	10:8,05	
10:3,6	10:8,75	
10:3,62	10:9,	
10:3,97	10:8,7	
10:4,1	10:9,5	
10:4,55	10:9,	
10:4,02	10:8,05	The 3d.
10:4,05	10:8,1	
10:4,22	10:9,1	
10:4,9	10:9,6	
10:3,97	10:9,17	4th.
10:4,52	10:9,5	
10:5,1	10:9,35	
10:4,55	10:9,45	5th.
10:4,9	10:9,3	
10:5,2	10:9,25	6th.
12.	13.	

In further confirmation of what is already delivered, I have adjoined a table, (TABLE I.) containing the refult of 27 fets of experiments, made and reduced in the manner above fpecified. What remains of the theory of underfhot wheels, will naturally follow from a comparifon of the different experiments together.

Maxims and Obfervations deduced from the foregoing Table of Experiments.

Maxim I. *That the virtual or effective head being the fame, the effect will be nearly as the quantity of water expended.*

This will appear by comparing the contents of the columns 4, 8, and 10, in the foregoing fets of experiments; as for

Example 1ft, taken from N°. 8. *and* 25, viz.

N°.	Virtual Head.	Water expended.	Effect.
8	7,29	161	328
25	7,29	355	785

Now the heads being equal, if the effects are proportioned to the water expended, we fhall have by maxim 1ft, 161 : 355 : : 328 : 723; but 723 falls fhort of 785, as it turns out in experiment, according to N°. 25, by 62; the effect therefore of

" pulfion fera augmenté; & experience jointe a la theorie a fait
" voir que dans ce cas, la force eft prefque double: de forte qui'il
" faut prendre le double de le fection du fil d'eau pour ce qui repond
" dans ce cas a le furface des aubes, poûrvu qu'elles foient affez
" larges pour recevoir ce fupplement de force. Car fi les aubes
" nétoient plus larges que le fil, on trait d'eau on ne devroit prendre
" que ne fimple fection, tout comme dans le premier cas, on l'aube
" toute entire eft pappee par l'eau."

N°.

N°. 25, compared with N°. 8, is greater than according to the present maxim in the ratio of 14 to 13.

The foregoing example, with four similar ones, are seen at one view in the following Table.

Examples.	N°. Table I.	Virtual Head.	Expence of Water.	Effect.	Comparison.	Variation.	Proportional Variation.
		Inch.	*b*				
1st	8 25	7,29 7,29	161 355	328 785	161 : 355 :: 328 : 723	62+	14 : 13
2d	13 18	10,5 10,5	285 357	975 1210	285 : 357 :: 975 : 1221	11−	121 : 122
3d	22 23	6,8 6,8	255 332	541 686	255 : 332 :: 541 : 704	18−	38 : 39
4th	21 24	4,7 4,7	228 262	317 385	228 : 262 :: 317 : 364	21+	18 : 17
5th	26 27	5,03 5,03	307 360	450 534	307 : 360 :: 450 : 531	3+	178 : 177

Hence

Hence therefore, in comparing different experiments, as some fall short, and others exceed the maximum, and all agree therewith, as near as can be expected, in an affair where so many different circumstances are concerned, we may, according to the laws of reasoning by induction, conclude the maxim true; *viz.* that the effects are nearly as the quantity of water expended.

Maxim II. *That the expence of water being the same, the effect will be nearly as the height of the virtual or effective head.*

This also will appear by comparing the contents of columns 4, 8, and 10, in any of the sets of experiments.

Example 1st, of N°. 2, *and* N°. 24, *viz.*

N°.	Virt. Head.	Expence.	Effect.
2	15	264,7	1266
24	4,7	262	385

Now as the expences are not quite equal, we must proportion one of the effects accordingly: thus

by maxim 1st,	262 :	264,7 : :	385 :	389
and by max. 2d,	15 :	4,7 : :	1266 :	397
		Difference - - - - -		8

The effect therefore of N°. 24, compared with N°. 2, is less than according to the present maxim in the ratio of 49 : 50.

The foregoing, and two other ſimilar examples, are compriſed in the following Table:

Examples.	N°. Tab. I.	Virtual Head.	Expence of Water.	Effect.	Compariſon.	Variation.	Proportional Variation.
1ſt	2 24	15 4,7	264,7 262	1266 385	Max. 1ſt, 262 : 264,7 :: 385 : 319 Max. 2d, 15 : 4,7 :: 1266 : 397	8 —	49 : 50
2d	1 10	15,85 3,55	275 114	1411 117	Max. 1ſt, 114 : 275 :: 117 : 282 Max. 2d, 15,85 : 3,55 :: 1411 : 316	34 —	8 : 9
3d	11 17	14,2 4,25	342 1675	1505 212	Max. 1ſt, 167,5 : 342 :: 212 : 433 Max. 2d, 14,2 : 4,25 :: 1505 : 450	17 —	25 : 26

Maxim

Maxim III. *That the quantity of water expended being the same, the effect is nearly as the square of its velocity.*

This will appear by comparing the contents of columns 3, 8, and 10, in any of the sets of experiments; as for

Example 1st, of N°. 2, *with* N°. 24, *viz.*

N°.	Turns in a min.	Expence.	Effect.
2	86	264,7	1266
24	48	262	385

The velocity being as the number of turns, we shall have,

$$\text{by max. 1st,}\quad 262 : 264{,}7 \;:\; 385 : 389$$

$$\text{and by max. 3d,}\quad \left\{\begin{matrix} 86^2 : 48^2 \\ 7396 : 2304 \end{matrix}\right\} : 1266 : 394$$

Difference - - - - - - 5

The effect therefore of N°. 24, compared with N°. 2, is less than by the present maxim in the ratio of 78 : 79.

The foregoing, and three other similar examples, are comprised in the following Table:

Examples.	N°. Table I.	Turns in a minute.	Expence of Water.	Effect.	Comparison.	Variation.	Proportional Variation.
1st	2 24	86 48	264,7 262	1266 385	Max. 1st, 262 : 264,7 :: 385 : 389 Max. 3d, $86^2 : 48^2$ = 7396 : 2304 :: 1266 : 394	5—	78 : 79
2d	1 10	88 42	275 114	1411 117	Max. 1st, 114 : 275 :: 117 : 282 Max. 3d, $88^2 : 42^2$ = 7744 : 1764 :: 1411 : 321	39—	7 : 8
3d	11 17	84 46	342 167,5	1505 212	Max. 1st, 167,5 : 342 :: 212 : 433 Max. 3d, $84^2 : 46^2$ = 7056 : 2116 :: 1505 : 451	18—	24 : 25
4th	18 21	72 48	357 228	1210 317	Max. 1st, 228 : 357 :: 317 : 496 Max. 3d, $72^2 : 48^2$ = 5184 : 2301 :: 1210 : 538	42—	12 : 13

Maxim

Maxim IV. *The aperture being the ſame, the effect will be nearly as the cube of the velocity of the water.*

This alſo will appear by comparing the contents of columns 3, 8, and 10; as for

Example 1ſt, of N°. 1, *and* N°. 10, *viz.*

N°.	Turns.	Expence.	Effect.
1	88	275	1411
10	42	114	117

Lemma. It muſt here be obſerved, that if water paſſes out of an aperture, in the ſame ſection, but with different velocities the expence will be proportional to the velocity; and therefore converſely, if the expence is not proportional to the velocity, the ſection of the water is not the ſame.

Now comparing the water diſcharged with the turns of N°. 1, and 10, we ſhall have 88 : 42 : : 275 : 131,2; but the water diſcharged by N°. 10, is only 114 lb. therefore, though the ſluice was drawn to the ſame height in N°. 10, as in N°. 1, yet the ſection of the water paſſing out, was leſs in N°. 10, than N°. 1, in the proportion of 114 to 131,2; conſequently had the effective aperture or ſection of the water been the ſame in N°. 10, as in N°. 1, ſo that 131,2 lb. of water had been diſcharged inſtead of 114, the effect would have been increaſed in the ſame proportion; that is

by the *Lemma*, 88 : 42 : : 275 : 131,2
by maxim 1ſt, 114 : 131,2 : : 117 : 134,5

$$\text{and by max. 4th, } \left\{\begin{matrix} 83^3 \\ 681472 \end{matrix}\right. \begin{matrix} : \\ : \end{matrix} \left.\begin{matrix} 42^3 \\ 74088 \end{matrix}\right\} :: 1411 : 153,5$$

Difference - - - 19

The

The effect therefore of 10, compared with N°. 1, is less than it ought to be by the present maxim in the ratio of 7 : 8.

The foregoing, and three other similar examples, are contained in the following Table

Examples.	N°. Tab. I.	Turns in a minute.	Expence of water.	Effect.	Comparison.	Variation.	Proportional Variation.
1ft	1 10	88 42	275 114	1411 117	Lemma. 88 : 42 :: 275 : 131,2 Max. 1. 114 : 131,2 :: 117 : 134,5 Max. 4. 88^3 : 42^3 :: 1411 : 153,5	19—	7 : 8
2d	11 17	84 46	342 167,5	1505 212	Lemma. 84 : 46 :: 342 : 187,3 Max. 1. 167,5 : 187,3 :: 212 : 237 Max. 4. 84^3 : 46^3 :: 1505 : 247	10—	23 : 24
3d	18 21	72 48	357 228	1210 317	Lemma. 72 : 48 :: 357 : 238 Max. 1. 228 : 238 :: 317 : 331 Max. 4. 72^3 : 48^3 :: 1210 : 355	24—	14 : 15
4th	22 24	68 48	359 262	1006 385	Lemma. 68 : 48 :: 359 : 253,4 Max. 1. 262 : 253,4 :: 285 : 372 Max. 4. 68^3 : 48^3 :: 1006 : 354	18+	20 : 19

Observations.

Obſervations.

Obſerv. 1ſt. On comparing column 2d and 4th, Tab. I. it is evident that the *virtual head* bears no certain proportion to the *head of water*; but that when the aperture is greater, or the velocity of the water iſſuing therefrom leſs, they approach nearer to a coincidence; and conſequently in the large openings of mills and ſluices, where great quantities of water are diſcharged from moderate heads, the head of water, and virtual head determined from the velocity, will nearly agree, as experience confirms.

Obſerv. 2d. Upon comparing the ſeveral proportions between the *power* and *effect* in column 11th, the moſt general is that of 10 to 3; the extremes 10 to 3,2 and 10 to 2,8; but as it is obſervable, that where the quantity of water, or the velocity thereof; that is, where the power is greateſt, the 2d term of the ratio is greateſt alſo: we may therefore well allow the proportion ſubſiſting in large works, as 3 to 1.

Obſerv. 3d. The proportions of *velocities* between the *water* and *wheel* in column 12, are contained in the limits of 3 to 1 and 2 to 1; but as the greater velocities approach the limit of 3 to 1, and the greater quantity of water approach to that of 2 to 1, the beſt general proportion will be that of 5 to 2.

Obſerv. 4th. On comparing the numbers in column 13, it appears, that there is no certain ratio between the *load* that the wheel will carry at its *maximum*, and what will totally ſtop it; but that they are contained within the limits of 20 to 19, and of 20 to 15; but as the effect approaches neareſt to the ratio of 20 to 15, or of 4 to 3, when the power is greateſt, whether by increaſe of velocity, or quantity of water, this ſeems to be the moſt applicable to large works; but as the load that a wheel ought to have, in order to work to the beſt advantage, can be

 aſſigned,

aſſigned, by knowing the effect it ought to produce, and the velocity it ought to have in producing it; the exact knowledge of the greateſt load it will bear, is of the leſs conſequence in practice.

It is to be noted, that in all the examples under the three laſt of the four preceding maxims, the effect of the leſſer power falls ſhort of its due proportion to the greater, when compared by its maxim; except the laſt example of maxim 4th: and hence, if the experiments are taken ſtrictly, we muſt infer, that the effects increaſe and diminiſh in an higher ratio than thoſe maxims ſuppoſe: but as the deviation is not very conſiderable, the greateſt being about 1-8th of the quantity in queſtion; and as it is not eaſy to make experiments of ſo compounded a nature with abſolute preciſion; we may rather ſuppoſe, that the leſſer power is attended with ſome friction, or works under ſome diſadvantage, which has not been duly accounted for; and therefore we may conclude, that theſe maxims will hold very nearly, when applied to works in large.

After the experiments above mentioned were tried, the wheel, which had originally 24 floats, was reduced to twelve; which cauſed a diminution in the effect, on account of a greater quantity of water eſcaping between the floats and the floor; but a circular ſweep being adapted thereto, of ſuch a length, that one float entered the curve before the preceding one quitted it, the effect came ſo near to the former, as not to give hopes of advancing it by increaſing the number of floats beyond 24 in this particular wheel.

PART

PART II.

Concerning OVERSHOT WHEELS.

Read before the Royal Society, May 24, 1759.

IN the former part of this eſſay, we have conſidered the impulſe of a confined ſtream, acting on *Underſhot Wheels.* We now proceed to examine the power and application of water, when acting by its *gravity* on *Overſhot Wheels.*

In reaſoning without experiment, one might be led to imagine, that however different the mode of application is; yet that whenever the ſame quantity of water deſcends through the ſame perpendicular ſpace, that the natural effective power would be equal: ſuppoſing the machinery free from friction, equally calculated to receive the full effect of the power, and to make the moſt of it: for if we ſuppoſe the height of a column of water to be 30 inches, and reſting upon a baſe or aperture of one inch ſquare, every cubic inch of water that departs therefrom will acquire the ſame velocity, or *momentum*, from the uniform preſſure of 30 cubic inches above it, that one cubic inch let fall from the top will acquire in falling down to the level of the aperture; *viz.* ſuch a velocity as, in a contrary direction, would carry it to the level from whence it fell *; one would therefore ſuppoſe, that a cubic inch of water, let fall through a ſpace of 30 inches, and there impinging upon another body, would be capable of producing an equal effect by colliſion, as if the ſame cubic inch had deſcended through the ſame ſpace with a ſlower motion, and produced its effects gradually: for in both caſes gravity acts upon an equal quantity of matter, through an equal

* This is a conſequence of the riſing of jets to the height of their reſervoirs nearly.

ſpace;

ſpace*; and conſequently, that whatever was the ratio between the power and effect in underſhot wheels, the ſame would obtain in overſhot, and indeed in all others: yet, however concluſive this reaſoning may ſeem, it will appear, in the courſe of the following deductions, that the effect of the gravity of deſcending bodies is very different from the effect of the ſtroke of ſuch as are *non elaſtic*, though generated by an equal mechanical power.

The alterations in the machinery already deſcribed, to accommodate the ſame for experiments on overſhot wheels, were principally as follows:

PLATE II. *Fig.* 2. The ſluice I *b* being ſhut down, the rod H I was unſcrewed and taken off.

The underſhot water-wheel was taken off the axis, and inſtead thereof an overſhot wheel of the ſame diameter was put into its place. *Note*, This wheel was two inches in the ſhroud or depth of the bucket; the number of the buckets was 36.

The ſtandards S and T, *Fig.* 1. were raiſed half an inch, ſo that the bottom of the wheel might be clear of ſtagnant water.

A trunk, for bringing the water upon the wheel, was fixed according to the dotted lines *f g*, *Fig.* 2. The aperture was adjuſted by a ſhuttle *h i*, which alſo cloſed up the outer end of the trunk, when the water was to be ſtopped.

Fig. 3. The ratchet *o o*, not being of one piece of metal with the ferrule *e e*, *i i* (though ſo deſcribed before, to prevent unneceſſary diſtinctions), was with its catch turned the contrary ſide; conſequently the moveable barrel would do its office equally, notwithſtanding the water-wheel, when at work, moved the contrary way.

* Gravity, it is true, acts a longer ſpace of time upon the body that deſcends ſlow than upon that which falls quick; but this cannot occaſion the difference in the effect: for an elaſtic body falling through the ſame ſpace in the ſame time, will, by colliſion upon another elaſtic body, rebound nearly to the height from which it fell; or, by communicating its motion, cauſe an equal one to aſcend to the ſame height.

Specimen

Specimen of a Set of Experiments.

Head 6 inches.

14½ ftrokes of the pump in a minute, 12 ditto = 80 lb.*
Weight of the fcale (being wet) 10½ oz.
Counterweight for 20 turns, befides the fcale, 3 oz.

No.	Weight in the Scale.	Turns.	Product.	Obfervations.
1	0 *lb.*	60	——	Threw moft part of the water out of the wheel.
2	1	56	——	
3	2	52	——	
4	3	49	147	Received the water more quietly.
5	4	47	188	
6	5	45	225	
7	6	42½	255	
8	7	41	287	
9	8	38½	308	
10	9	36½	328½	
11	10	35½	355	
12	11	32¾	360½	
13	12	31¼	375	
14	13	28½	370½	
15	14	27½	385	
16	15	26	390	
17	16	24½	392	
18	17	22¾	386¾	
19	18	21¾	391½	
20	19	20¾	394¼	maximum.
21	20	19¾	395	
22	21	18¼	388¼	
23	22	18	396	Worked irregular.
24	23	Overfet by its load.		

* The fmall difference, in the value of 12 ftrokes of the pump, from the former experiments, was owing to a fmall difference in the length of the ftroke, occafioned by the warping of the wood.

Reduction

Reduction of the preceding Specimen.

In these experiments the head being 6 inches, and the height of the wheel 24 inches, the whole descent will be 30 inches: the expence of water was $14\frac{1}{2}$ strokes of the pump in a minute, whereof 12 contained 80 lb.; therefore the water expended in a minute was $96\frac{2}{3}$ lb. which multiplied by 30 inches, gives the *power* = 2900.

If we take the 20th experiment for the *maximum*, we shall have $20\frac{3}{4}$ turns in a minute, each of which raised the weight $4\frac{1}{2}$ inches, that is, 93,37 inches in a minute. The weight *in* the scale was 19 lb. the weight of the scale $10\frac{1}{2}$ oz.; the counterweight 3 oz. in the scale, which, with the weight of the scale $10\frac{1}{2}$ oz. makes in the whole $20\frac{1}{2}$ lb. which is the whole resistance or load: this, multiplied by 93,37 inches, makes 1914 for the effect.

The *ratio* therefore of the *power* and *effect* will be as 2900 : 1914, or as 10 : 6,6, or as 3 : 2 nearly.

But if we compute the power from the height of the wheel only, we shall have $96\frac{2}{3}$ lb. multiplied by 24 inches = 2320 for the *power*, and this will be to the *effect* as 2320 : 1914, or as 10 : 82, or as 5 : 4 nearly.

The reduction of this specimen is set down in N°. 9, of the following Table; and the rest were deduced from a similar set of experiments, reduced in the same manner.

TABLE

Table II. *containing the Result of Sixteen Sets of Experiments on Overshot Wheels.*

N°.	Whole descent.	Water expended in a minute.	Turns at the maximum in a min.	Weight raised at the maximum.	Power of the whole descent.	Power of the wheel.	Effect.	Ratio of the whole power and effect.	Ratio of power of the wheel and effect.	Mean ratio.
	Inch.	*lb.*		*lb.*						
1	27	30	19	$6\frac{1}{2}$	810	720	556	10 : 6,9	10 : 7,7	Medium 10:8,1
2	27	$56\frac{2}{3}$	$16\frac{1}{4}$	$14\frac{1}{2}$	1530	1360	1060	10 : 6,9	10 : 7,8	
3	27	$56\frac{2}{3}$	$20\frac{3}{4}$	$12\frac{1}{2}$	1530	1360	1167	10 : 7,6	10 : 8,4	
4	27	$63\frac{1}{3}$	$20\frac{1}{2}$	$13\frac{1}{2}$	1710	1524	1245	10 : 7,3	10 : 8,2	
5	27	$76\frac{2}{3}$	$21\frac{1}{2}$	$15\frac{1}{2}$	2070	1840	1500	10 : 7,3	10 : 8,2	
6	$28\frac{1}{2}$	$73\frac{1}{3}$	$18\frac{3}{4}$	$17\frac{1}{2}$	2090	1764	1476	10 : 7,	10 : 8,4	10:8,2
7	$28\frac{1}{2}$	$96\frac{2}{3}$	$20\frac{1}{4}$	$20\frac{1}{2}$	2755	2320	1868	10 : 6,8	10 : 8,	
8	30	90	20	$19\frac{1}{2}$	2700	2160	1755	10 : 6,5	10 : 8,1	10 : 8,2
9	30	$96\frac{2}{3}$	$20\frac{3}{4}$	$20\frac{1}{2}$	2900	2320	1914	10 : 6,6	10 : 8,2	
10	30	$113\frac{1}{3}$	21	$23\frac{1}{2}$	3400	2720	2221	10 : 6,5	10 : 8,2	
11	33	$56\frac{2}{3}$	$20\frac{1}{4}$	$13\frac{1}{2}$	1870	1360	1230	10 : 6,6	10 : 9,	10 : 8,5
12	33	$106\frac{2}{3}$	$22\frac{1}{4}$	$21\frac{1}{2}$	3520	2560	2153	10 : 6,1	10 : 8,4	
13	33	$146\frac{2}{3}$	23	$27\frac{1}{2}$	4840	3520	2846	10 : 5,9	10 : 8,1	
14	35	65	$19\frac{3}{4}$	$16\frac{1}{2}$	2275	1560	1466	10 : 6,5	10 : 9,4	10 : 8,5
15	35	120	$21\frac{1}{2}$	$25\frac{1}{2}$	4200	2880	2467	10 : 5,9	10 : 8,6	
16	35	$163\frac{1}{2}$	25	$26\frac{1}{2}$	5728	3924	2981	10 : 5,2	10 : 7,6	
1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11

Observations and Deductions from the foregoing Experiments.

I. *Concerning the Ratio between the Power and Effect of Overshot Wheels.*

The effective power of the water must be reckoned upon the whole descent; because it must be raised that height, in order to be in a condition of producing the same effect a second time.

The

The ratios between the *powers* so estimated, and the *effects* at the *maximum* deduced from the several sets of experiments, are exhibited at one view in column 9. of Table II.; and from hence it appears, that those ratios differ from that of 10 to 7,6 to that of 10 : 5,2, that is, nearly from 4 : 3 to 4 : 2. In those experiments where the heads of water and quantities expended are least, the proportion is nearly as 4 : 3; but where the heads and quantities are greatest, it approaches nearer to that of 4 : 2; and by a medium of the whole, the ratio is that of 3 : 2 nearly. We have seen before, in our observations upon the effects of undershot wheels, that the general ratio of the power to the effect, when greatest was 3 : 1; *the effect therefore of overshot wheels, under the same circumstances of quantity and fall, is at a medium double to that of the undershot:* and, as a consequence thereof, *that non-elastic bodies, when acting by their impulse or collision, communicate only a part of their original power*; the other part being spent in changing their figure in consequence of the stroke.

The powers of water computed from the height of the wheel, only compared with the effects, as in column 10, appear to observe a more constant ratio: for if we take the medium of each class, which is set down in column 11, we shall find the extremes to differ no more than from the ratio of 10 : 8,1 to that of 10 : 8,5; and as the second term of the ratio gradually increases from 8,1 to 8,5, by an increase of head from 3 inches to 11, the excess of 8,5 above 8,1 is to be imputed to the superior impulse of the water at the head of 11 inches above that of 3 inches: so that if we reduce 8,1 to 8, on account of the impulse of the 3 inch head, *we shall have the ratio of the power, computed upon the height of the wheel only, to the effect at a maximum as 10 : 8, or as 5 : 4 nearly:* and from the quality of the ratio between power and effect, subsisting where the constructions are similar, we must infer, *that the effects, as well as the powers, are as the quantities of water and perpendicular heights multiplied together respectively.*

II. Con-

II. *Concerning the most proper Height of the Wheel in Proportion to the whole Descent.*

We have already seen, from the preceding observation, that the effect of the same quantity of water, descending through the same perpendicular space, is double, when acting by its gravity upon an overshot wheel, to what the same produces when acting by its impulse upon an undershot. It also appears, that by increasing the head from 3 inches to 11, that is, the whole descent, from 27 inches to 35, or in the ratio of 7 to 9 nearly, the effect is advanced no more than in the ratio of 8,1 to 8,4 that is, as 7 : 7,26; and consequently the increase of effect as not 1-7th of the increase of perpendicular height. Hence it follows, *that the higher the wheel is in proportion to the whole descent, the greater will be the effect*; because it depends less upon the impulse of the head, and more upon the gravity of the water in the buckets: and if we consider how obliquely the water issuing from the head must strike the buckets, we shall not be at a loss to account for the little advantage that arises from the impulse thereof: and shall immediately see of how little consequence this impulse is to the effect of an overshot wheel. However, as every thing has its limits, so has this: for thus much is desirable *that the water should have somewhat greater velocity, than the circumference of the wheel, in coming thereon*; otherwise the wheel will not only be retarded, by the buckets striking the water, but thereby dashing a part of it over, so much of the power is lost.

The velocity that the circumference of the wheel ought to have, being known by the following deductions, the head requisite to give the water its proper velocity is easily computed from the common rules of hydrostatics; and will be found much less than what is generally practised.

III. Con-

III. *Concerning the Velocity of the Circumference of the Wheel, in order to produce the greatest Effect.*

If a body is let fall freely from the surface of the head to the bottom of the descent, it will take a certain time in falling; and in this case the whole action of gravity is spent in giving the body a certain velocity: but if this body in falling is made to act upon some other body, so as to produce a mechanical effect, the falling body will be retarded; because a part of the action of gravity is then spent in producing the effect, and the remainder only giving motion to the falling body: and therefore *the slower a body descends, the greater will be the portion of the action of gravity applicable to the producing a mechanical effect*; and in consequence the greater that effect may be.

If a stream of water falls into the bucket of an overshot wheel, it is there retained until the wheel by moving round discharges it: of consequence the slower the wheel moves, the more water each bucket will receive: so that what is lost in speed, is gained by the pressure of a greater quantity of water acting in the buckets at once: and, if considered only in this light, the mechanical power of an overshot wheel to produce effects will be equal whether it moves quick or slow: but if we attend to what has been just now observed of the falling body, it will appear that so much of the action of gravity, as is employed in giving the wheel and water therein a greater velocity, must be subtracted from its pressure upon the buckets; so that, though the product made by multiplying the number of cubic inches of water acting in the wheel at once by its velocity will be the same in all cases; yet, as each cubic inch, when the velocity is *greater* does not press so much upon the bucket as when it is *less*, the power of the water to produce effects will be greater in the less velocity than in the greater: and hence we are led to this general rule, *that*, cæteris paribus, *the less the velocity of the wheel, the greater will be the effect thereof.* A confirmation of this

this doctrine, together with the limits it is subject to in practice, may be deduced from the foregoing specimen of a set of experiments.

From these experiments it appears that, when the wheel made about 20 turns in a minute, the effect was, near upon, the greatest. When it made 30 turns, the effect was diminished about $\frac{1}{20}$ part; but that when it made 40, it was diminished about $\frac{1}{4}$; when it made less than $18\frac{1}{4}$, its motion was irregular; and when it was loaded so as not to admit its making 18 turns, the wheel was overpowered by its load.

It is an advantage in practice, that the velocity of the wheel should not be diminished further than what will procure some solid advantage in point of power: because, *cæteris paribus*, as the motion is slower, the buckets must be made larger; and the wheel being more loaded with water, the stress upon every part of the work will be increased in proportion: *The best velocity for practice therefore will be such, as when the wheel here used made about 30 turns in a minute; that is, when the velocity of the circumference is a little more than 3 feet in a second.*

Experience confirms, that this velocity of 3 feet in a second is applicable to the highest overshot wheels, as well as the lowest; and all other parts of the work being properly adapted thereto, will produce very nearly the greatest effect possible: however this also is certain from experience, that *high wheels may deviate further from this rule, before they will lose their power, by a given aliquot part of the whole, than low ones can be admitted to do*; for a wheel of 24 feet high may move at the rate of six feet per second without losing any considerable part of its power*;

* The 24 feet wheel going at 6 feet in a second, seems owing to the small proportion that the head (requisite to give the water the proper velocicy of the wheel) bears to the whole height.

and on the other hand, I have ſeen a wheel of 33 feet high, that has moved very ſteadily and well with a velocity but little exceeding 2 feet.

IV. *Concerning the Load for an Overſhot Wheel, in Order that it may produce a Maximum.*

The maximum load for an overſhot wheel, is that which reduces the circumferences of the wheel to its proper velocity; and this will be known, by dividing the effect it ought to produce in a given time by the ſpace intended to be deſcribed by the circumference of the wheel in the ſame time: the quotient will be the reſiſtance overcome at the circumference of the wheel; and is equal to the load required, the friction and reſiſtance of the machinery included.

V. *Concerning the greateſt poſſible Velocity of an Overſhot Wheel.*

The greateſt velocity that the circumference of an overſhot wheel is capable of, depends jointly upon the diameter or *height* of the wheel, and the velocity of falling bodies; for it is plain that the velocity of the circumference can never be greater, than to deſcribe a ſemi-circumference, while a body let fall from the top of the wheel will deſcend through its diameter; nor indeed quite ſo great, as a body deſcending through the ſame perpendicular ſpace cannot perform the ſame in ſo ſmall a time when paſſing through a ſemi-circle, as would be done in a perpendicular line. Thus, if a wheel is 16 feet 1 inch high, a body will fall through the diameter in one ſecond: this wheel therefore can never arrive at a velocity equal to the making one turn in two ſeconds; but, in reality, an overſhot wheel can never come near this velocity; for when it acquires a certain ſpeed, the greateſt part of the water is prevented from entering the buckets;

buckets; and the reſt, at a certain point of its deſcent, is thrown out again by the centrifugal force. This appears to have been the caſe in the three firſt experiments of the foregoing ſpecimen; but as the velocity, when this begins to happen, depends upon the form of the buckets, as well as other circumſtances, *the utmoſt velocity of overſhot wheels is not to be determined generally*: and, indeed, it is the leſs neceſſary in practice, as it is in this circumſtance incapable of producing any *mechanical effect*, for reaſons already given.

VI. *Concerning the greateſt Load that an Overſhot Wheel can overcome.*

The greateſt load an overſhot wheel will overcome, conſidered abſtractedly, is unlimited or infinite: for as the buckets may be of any given capacity, the more the wheel is loaded, the ſlower it turns; but the ſlower it turns, the more will the buckets be filled with water; and conſequently though the diameter of the wheel and quantity of water expended, are both limited, yet no reſiſtance can be aſſigned, which it is not able to overcome: but in practice we always meet with ſomething that prevents our getting into infiniteſimals; for when we really go to work to build a wheel, the buckets muſt neceſſarily be of ſome given capacity; and conſequently *ſuch a reſiſtance will ſtop the wheel, as is equal to the effort of all the buckets in one ſemi-circumference filled with water.*

The ſtructure of the buckets being given; the quantity of this effort may be aſſigned; but is not of much conſequence to the practice, as in this caſe alſo the wheel loſes its power; for though here be the exertion of gravity upon a given quantity of water, yet being prevented by a counterbalance from moving, is capable of producing no *mechanical effect*, according to our definition. But, in reality, an overſhot wheel generally ceaſes to be uſeful before it is loaded to that pitch; for *when it meets with*

 ſuch

such a resistance as to diminish its velocity to a certain degree, its motion becomes irregular; yet this never happens until the velocity of the circumference is less than 2 feet per second, where the resistance is equable, as appears not only from the preceding specimen, but from experiments on larger wheels.

SCHOLIUM.

Having now examined the different effects of the power of water; when acting by its *impulse*, and by its *weight*, under the titles of *undershot* and *overshot* wheels; we might naturally proceed to examine the effects when the impulse and weight are combined, as in the several kinds of *breast wheels, &c.* but, what has been already delivered being carefully attended to, the application of the same principles in these mixt cases will be easy, and reduce what I have to say on this head into a narrow compass: for all kinds of wheels where the water cannot descend through a given space, unless the wheel moves therewith, are to be considered of the nature of an overshot wheel, according to the perpendicular height that the water descends from; and all those that receive the impulse or shock of the water, whether in an horizontal, perpendicular, or oblique direction, are to be considered as undershots. And therefore a wheel, which the water strikes at a certain point below the surface of the head, and after that descends in the arch of a circle, pressing by its gravity upon the wheel; *the effect of such a wheel will be equal to the effect of an undershot, whose head is equal to the difference of level between the surface of the water in the reservoir and the point where it strikes the wheel, added to that of an overshot, whose height is equal to the difference of level between the point where it strikes the wheel and the level of the tail-water.* It is here supposed, that the wheel receives the shock of the water at right angles to its radii; and that the velocity of its circumference is properly adapted to receive the utmost advantage of both these powers; otherwise a reduction must be made on that account.

Many

Many obvious and confiderable improvements upon the common practice naturally offer themfelves, from a due confideration of the principles here eftablifhed, as well as many popular errors fhow themfelves in view: but as my prefent purpofe extends no farther than the laying down fuch general rules as will be found to anfwer in practice, I leave the particular application to the intelligent artift, and to the curious in thefe matters.

PART III.

On the Construction *and* Effects *of* Windmill Sails.

Read before the Royal Society 31 *May and* 14 *June* 1759.

In trying experiments on windmill fails, the wind itfelf is too uncertain to anfwer the purpofe, we muft therefore have recourfe to an artificial wind.

This may be done two ways; either by caufing the air to move againft the machine, or the machine to move againft the air. To caufe the air to move againft the machine, in a fufficient volume, with fteadinefs and the requifite velocity, is not eafily put in practice: To carry the machine forward in a right line againft the air, would require a larger room than I could conveniently meet with. What I found moft practicable, therefore, was to carry the axis, whereon the fails were to be fixed, progreffively round in the circumference of a large circle. Upon this idea* a machine was conftructed, as follows.

Plate

* Some years ago Mr. Roufe, an ingenious gentleman of Harborough, in Leicefterfhire, fet about trying experiments on the velocity of the wind, and force thereof upon plain furfaces and windmill-fails; and, much about the fame time, Mr. Ellicott contrived a machine for the ufe of the late celebrated Mr. B. Robins, for trying the refiftance of plain furfaces moving through the air. The machines of both thefe gentlemen were much alike, though at that time totally unacquainted with each other's inquiries. But it often happens, that when two perfons think juftly upon the fame fubject, their experiments are alike. This machine was alfo built upon the fame idea as the foregoing; but differed in having the hand for the firft mover, with a pendulum for its regulator, inftead of a weight, as

PLATE III. Fig. 1.

A B C is a pyramidical frame for ſupporting the moving parts.

D E is an upright axis, whereon is framed

F G, an arm for carrying the ſails at a proper diſtance from the centre of the upright axis.

H is a barrel upon the upright axis, whereon is wound a cord; which, being drawn by the hand, gives a circular motion to the axis, and to the arm F G; and thereby carries the axis of the ſails in the circumference of a circle, whoſe radius is D I, cauſing thereby the ſails to ſtrike the air, and turn round upon their own axis.

At L is fixed the end of a ſmall line, which paſſing through the pullies M N O, terminates upon a ſmall cylinder or barrel upon the axis of the ſails, and, by winding thereon, raiſes

P the ſcale, wherein the weights are placed for trying the power of the ſails. This ſcale, moving up and down in the direction of the upright axis, receives no diſturbance from the circular motion.

Q R two parallel pillars ſtanding upon the arm F G, for the purpoſe of ſupporting and keeping ſteady the ſcale P; which is kept from ſwinging by means of

as in the former; which was certainly beſt for the purpoſes of meaſuring the impulſe of the wind, or reſiſtance of plains; but the latter is more applicable to experiments on windmill ſails; becauſe every change of poſition of the ſame ſails will occaſion their meeting the air with a different velocity, though urged by the ſame weight.

S T, two ſmall chains, which hang looſely round the two pillars.

W is a weight for bringing the centre of gravity of the moveable part of the machine into the centre of motion of the axis D E.

V X is a pendulum, compoſed of two balls of lead, which are moveable upon a wooden rod, and thereby can be ſo adjuſted, as to vibrate in any time required. This pendulum hangs upon a cylindrical wire, whereon it vibrates, as on a rolling axis.

Y is a perforated table for ſupporting the axis of the pendulum.

Note, The pendulum being ſo adjuſted, as to make two vibrations in the time that the arm F G is intended to make one turn; the pendulum being ſet a vibrating, the experimenter pulls by the cord Z, with ſufficient force to make each half revolution of the arm to correſpond with each vibration, as equal as poſſible, during the number of vibrations that the experiment is intended to be continued. A little practice renders it eaſy to give motion thereto with all the regularity that is neceſſary.

Specimen of a Set of Experiments.

Radius of the ſails — —	21 inches
Length of ditto in the cloth — —	18
Breadth of ditto — — —	5,6
* { Angle at the extremity — —	10 degrees
{ Ditto at the greateſt inclination —	25

20

* In all the following experiments the angle of the ſails is accounted from the plain of their motion; that is, when they ſtand

at

20 turns of the sails raised the weight — 11,3 inches.

Velocity of the centre of the sails, in the circumference of the great circle, in a second — — — } 6 feet 0 inches.

Continuance of the experiment — 52 seconds.

No.	Wt. in the scale.	Turns.	Product.
1	0 *lb.*	108	0
2	6	85	510
3	$6\frac{1}{2}$	81	$526\frac{1}{2}$
4	7	78	546
5	$7\frac{1}{2}$	73	$547\frac{1}{2}$ maximum
6	8	65	520
7	9	0	0

N. B. The weight of the scale and pulley was 3 oz.; and that 1 oz. suspended upon one of the radii, at $12\frac{1}{2}$ inches from the centre of the axis, just overcame the friction scale and load of $7\frac{1}{2}$ lb.; and placed at $14\frac{1}{10}$ inches, overcame the same resistances with 9 lb. in the scale.

Reduction of the preceding Specimen.

N°. 5. being taken for the maximum, the weight in the scale was 7 lb. 8 oz. which, with the weight of the scale and pulley 3 oz. makes 7 lb. 11 oz. equal to 123 oz.; this added to the friction of the machinery, the sum is the whole resistance*. The

at right angles to the axis, their angle is denoted 0°, this notation being agreeable to the language of practitioners, who call the angle so denoted, the weather of the sail; which they denominate greater or less, according to the quantity of this angle.

* The resistance of the air is not taken into the account of resistance, because it is inseparable from the application of the power.

friction

friction of the machinery is thus deduced: Since 20 turns of the sails raised the weight 11,3 inches, with a double line, the radius of the cylinder will be .18 of an inch; but had the weight been raised by a single line, the radius of the cylinder being half the former, *viz.* .09, the resistance would have been the same: we shall therefore have this analogy; as half the radius of the cylinder, is to the length of the arm where the small weight was applied; so is the weight applied to the arm, to a fourth weight, which is equivalent to the sum of the whole resistance together; that is, .09 : 12,5 : : 1 oz. : 139 oz. this exceeds 123 oz. the weight in the scale, by 16 oz. or 1 lb. which is equivalent to the friction; and which, added to the above weight of 7 lb. 11 oz. makes 8 lb. 11 oz.=8,69 lb. for the sum of the whole resistance; and this, multiplied by 73 turns, makes a product of 634, which may be called the representative of the *effect* produced.

In like manner, if the weight 9 lb. which caused the sails to rest after being in motion, be augmented by the weight of the scale and its relative friction, it will become 10,37 lb. The result of this specimen is set down in N°. 12. of Table III. and the result of every other set of experiments therein contained were made and reduced in the same manner.

TABLE III.

etts of Experiments on Windmill Sails
ons, and Quantities of Surfaces.

Load at the maximum.	Greatest load.	Product.	Quantity of surface.	Ratio of greatest velocity to the velocity at maximum.	Ratio of greatest load to the load at maximum.	Ratio of surface to the product.
lb.	*lb.*		*Sq In*			
7,56	12,59	318	404	10:7	10:6	10:7,9
6,3	7,56	441	404		10:8,3	10:10,1
6,72	8,12	464	404	10:6,6	10:8,3	10:10,15
7,0	9,81	462	404	10:7,	10:7,1	10:10,15
7,0		462	404			10:11,4
7,35		518	404			10:12,8
8,3		527	404			10:13,
4,75	5,31	442	404	10:7,7	10:8,9	10:11,
7,0	8,12	553	404	10:6,6	10:8,6	10:13,7
7,5	8,12	585	404		10:9,2	10:14,5
8,3	9,81	639	404	10:6,8	10:8,5	10:15,8
8,69	10,37	634	404	10:6,8	10:8,4	10:15,7
8,41	10,94	580	404	10:6,6	10:7,7	10:14,4
10,65	12,59	799	505	10:6,1	10:8,5	10:15,8
11,08	13,69	820	505	10:6,3	10:8,1	10:16,2
12,09	14,23	799	505	10:5,8	10:8,4	10:15,8
12,09	14,78	762	505	10:6,6	10:8,2	10:15,1
16,42	27,87	1059	854	10:6,1	10:5,9	10:12,4
18,06		1165	1146	10:5,9		10:10,1
6.	7.	8.	9.	10.	11.	12.

e page 43.

Obſervations and Deductions from the preceding Experiments.

I. *Concerning the beſt Form and Poſition of Windmill Sails.*

In Table III. N°. 1. is contained the reſult of a ſet of experiments upon ſails ſet at the angle which the celebrated Monſ. Parint, and ſucceeding geometricians for many years, held to be the beſt; *viz.* thoſe whoſe planes make an angle 55° nearly with the axis; the complement whereof, or angle that the plane of the ſail makes with the plane of their motion, will therefore be 35°, as ſet down in col. 2. and 3. Now if we multiply their number of turns by the weight they lifted, when working to the greateſt advantage, as ſet down in columns 5. and 6. and compare this product (col. 8.) with the other products contained in the ſame column, inſtead of being the greateſt, it turns out the leaſt of all the reſt. But if we ſet the angle of the ſame planes at ſomewhat leſs than half the former, or at any angle from 15° to 18°, as in N°. 3. and 4. that is, from 72° to 75° with the axis, the product will be increaſed in the ratio of 31 : 45; and this is the angle moſt commonly made uſe of by practitioners, when the ſurfaces of the ſails are planes.

If nothing more was intended than to determine the moſt efficacious angle to make a mill acquire motion from a ſtate of reſt, or to prevent it from paſſing into reſt from a ſtate of motion, we ſhall find the poſition of N°. 1. the beſt; for if we conſult col. 7. which contains the leaſt weights, that would make the ſails paſs from motion to reſt, we ſhall find that of N°. 1. (relative to the quantity of cloth) the greateſt of all. But if the ſails are intended, with given dimenſions, to produce the greateſt effect poſſible in a given time, we muſt entirely reject thoſe of N°.

N°. 1. and, *if we are confined to the use of planes, conform ourselves to some angle between* N°. 3. *and* 4. *that is, not less than* 72°, *or greater than* 75°, *with the axis.*

The late celebrated Mr. Maclaurin has judiciously distinguished between the action of the wind upon a sail at rest, and a sail in motion; and, in consequence, as the motion is more rapid near the extremities than towards the centre, that the angle of the different parts of the sail, as they recede from the centre, should be varied. For this purpose he has furnished us with the following theorem*. "Suppose the velocity of the wind to "be represented by a, and the velocity of any given part of the "sail to be denoted by c; then the effort of the wind upon that "part of the sail will be greatest when the tangent of the angle, "in which the wind strikes it, is to radius as

"$\sqrt{2+\frac{9cc}{4aa}}+\frac{3c}{2a}$ to 1." This theorem then assigns the law, by which the angle is to be varied according to the velocity of each part of the sail to the wind: but as it is left undetermined what velocity any one given part of the sail ought to have in respect to the wind, the angle that any one part of the sail ought to have, is left undetermined also; so that we are still at a loss for the proper *data* to apply the theorem. However, being willing to avail myself thereof, and considering that any angle from 15° to 18° was best suited to a plane, and of consequence to the best mean angle, I made the sail, at the middle distance between the centre and the extremity, to stand at an angle of 15° 41′ with the plane of the motion; in which case the velocity of that part of the sail, when loaded to a *maximum*, would be equal to that of the wind, or $c=a$. This being determined, the rest were inclined according to the theorem, as follows:

* Maclaurin's account of Sir Isaac Newton's philosophical discoveries, p. 176, art. 29.

Angle

Parts of the radius from the centre.		Angle with the axis.	Angle of weather.	
$\frac{1}{6}$	$c=\frac{1}{3}a$	63° 26′	26° 34′	
$\frac{2}{6}$	$c=\frac{2}{3}a$	69 54	20 6	
$\frac{1}{2}$	$c=a$	74 19	15 41	middle.
$\frac{2}{3}$	$c=1\frac{1}{3}a$	77 20	12 40	
$\frac{5}{6}$	$c=1\frac{2}{3}a$	79 27	10 33	
1	$c=2a$	81 0	9 0	extremity.

The reſult hereof was according to N°. 5. being nearly the ſame as the plane ſails, in their beſt poſition: but being turned round in their ſockets, ſo that every part of each ſail ſtood at an angle of 3°, and afterwards of 6°, greater than before, that is, their extremities being moved 9° to 12° and 15°, the products were advanced to 518 and 527 reſpectively. Now from the ſmall difference between thoſe two products, we may conclude, that they were nearly in their beſt poſition, according to N°. 7. or ſome angle between that and N°. 6: but from theſe, as well as the plane ſails and others, we may alſo conclude, that *a variation in the angle of a degree or two makes very little difference in the effect, when the angle is near upon the beſt.*

It is to be obſerved, that a ſail inclined by the preceding rule will expoſe a convex ſurface to the wind: whereas the Dutch, and all our modern mill-builders, though they make the angle to diminiſh, in receding from the centre towards the extremity, yet conſtantly do it in ſuch manner, as that the ſurface of the ſail may be concave towards the wind. In this manner the ſails made uſe of in N°. 8, 9, 10, 11, 12, and 13, were conſtructed; the middle of the ſail making an angle with the extreme bar of 12°; and the greateſt angle (which was about $\frac{1}{3}$ of the radius from the centre) of 15° therewith. Thoſe ſails being tried in various poſitions, the beſt appears to be that of N°. 11. where the extremities ſtood at an angle of 7°$\frac{1}{2}$ with the plane of motion, the product

product being 639: greater than that of those made by the theorem in the ratio of 9 : 11, and double to that of N°. 1; and this was the greatest product that could be procured without an augmentation of surface. Hence it appears, that *when the wind falls upon a concave surface, it is an advantage to the power of the whole, though every part, taken separately, should not be disposed to the best advantage* *.

Having thus obtained the best position of the sails, or manner of weathering, as it is called by the workmen, the next point was to try what advantage could be made by an addition of surface upon the same radius. For this purpose, the sails made use of had the same weather as those N°. 8. to 13, with an addition to the leading side of each of a triangular cloth, whose height was equal to the height of the sail, and whose base was equal to half the breadth: of consequence the increase of surface upon the whole was one-fourth part, or as 4 : 5. Those sails, by being turned round in their sockets, were tried in four different positions, specified in N°. 14, 15, 16, and 17; from whence it appears, that the best was when every part of the sail made a greater angle by 2°½, with the plane of the motion, than those without the addition, as appears by N°. 15. the product being 820;

* By several trials in large I have found the following angles to answer as well as any. The radius is supposed to be divided into 6 parts and 1-6th, reckoning from the centre, is called 1, the extremity being denoted 6.

N°.	Angle with the axis.	Angle with the plane of the motion.
1	72°	18°
2	71	19
3	72	18 middle.
4	74	16
5	77½	12½
6	83	7 extremity.

820: this exceeds 639 more than in the ratio of 4 : 5, or that of the increafe of cloth. Hence it appears, that *a broader fail requires a greater angle*; and *that when the fail is broader at the extremity, than near the centre, this fhape is more advantageous than that of a parallelogram**.

Many have imagined, that the more fail, the greater the advantage, and have therefore propofed to fill up the whole area: and by making each fail a fector of an ellipfis, according to Monfieur Parint, to intercept the whole cylinder of wind, and thereby to produce the greateft effect poffible.

We have therefore proceeded to inquire how far the effect could be increafed by a further enlargement of the furface, upon the fame radius of which N°. 18 and 19 are fpecimens. The furfaces indeed were not made planes, and fet at an angle of 35°, as Parint propofed; becaufe, from N°. 1. we learn, that this pofition has nothing to do, when we intend them to work to the greateft advantage. We therefore gave them fuch an angle as the preceding experiments indicated for fuch fort of fails, *viz.* 12° at the extremity, and 22° for the greateft weather. By N°. 18. we have the product 1059, greater than N°. 15. in the ratio of 7 : 9; but then the augmentation of cloth is almoft 7 : 12. By N°. 19. we have the product 1165, that is greater than N°. 15. as 7 : 10; but the augmentation of cloth is nearly as 7 : 16; confequently had the fame quantity of cloth as in

* The figure and proportion of the enlarged fails, which I have found beft to anfwer in large, are reprefented in the figure, Plate III, where the extreme bar is 1-3d of the radius (or whip, as it is called by the workmen), and is divided by the whip in the proportion of 3 to 5. The triangular or leading fail is covered with board from the point downwards 1-3d of its height, the reft with cloth as ufual. The angles of weather in the preceding note are beft for the enlarged fails alfo; for in practice it is found, that the fails had better have too little than too much weather.

N°.

N°. 18, been difpofed in a figure fimilar to that of N°. 15, inftead of the product being 1059, we fhould have had the product 1386; and in N°. 19, inftead of the product 1165, we fhould have had a product of 1860; as will be further made appear in the courfe of the following deductions. Hence it appears, that beyond a certain degree, the more the area is crouded with fail, the lefs effect is produced in proportion to the furface: and, by purfuing the experiments ftill further, I found, that though in N°. 19, the furface of all the fails together were not more than 7-8ths of the circular area containing them, yet a further addition rather diminifhed than increafed the effect. *So that when the whole cylinder of wind is intercepted, it does not then produce the greateft effect for want of proper interftices to efcape.*

It is certainly defirable that the fails of windmills fhould be as fhort as poffible; but at the fame time it is equally defirable, the quantity of cloth fhould be the leaft that may be, to avoid damage by fudden fquals of wind. The beft ftructure, therefore, for large mills, is that where the quantity of cloth is the greateft, in a given circle, that can be: on this condition, that the effect holds out in proportion to the quantity of cloth; for otherwife the effect can be augmented in a given degree by a leffer increafe of cloth upon a larger radius, than would be required, if the cloth was increafed upon the fame radius. The moft ufeful figure, therefore for practice, is that of N°. 9 or 10, as has been experienced upon feveral mills in large.

TABLE

TABLE IV. *Containing the Reſult of ſix Sets of Experiments, made for determining the Difference of Effect, according to the different Velocity of the Wind.*

N. B. The ſails were of the ſame ſize and kind as thoſe of N°. 10, 11, and 12, Tab. III. Continuance of the Experiment one minute.

N°.	Angle at the extremity.	Velocity of the wind in a ſecond.	Turns of the ſails unloaded.	Turns of the ſails at maximum.	Load at the maximum.	Greateſt load.	Product.	Maximum load for the half velocity.	Turns of the ſails therewith.	Product of leſſer load and greater velocity.	Ratio of the two products.	Ratio of the greateſt velocity to the velocity at a maxim^m.	Ratio of the greateſt load to the load at a maximum.
		f. in.			*lb.*	*lb.*							
1	5°	4 4½	96	66	4,47	5,37	295	—	—	—	— —	10 : 6,9	10 : 8,3
2	5	8 9	207	122	16,42	18,06	2003	4,47	180	805	10 : 27,3	10 : 5,9	10 : 9,1
3	7½	4 4½	—	65	4,62	—	300	—	—	—	— —	—	—
4	7½	8 9	—	130	17,52	—	2278	4,62	180	832	10 : 27,8	—	—
5	10	4 4½	91	61	5,03	5,87	307	—	—	—	— —	10 : 6,7	10 : 8,5
6	10	8 9	178	110	18,61	21,34	2047	5,03	158	795	10 : 26	10 : 6,2	10 : 8,7
1	2	3	4	5	6	7	8	9	10	11	12	13	14

II. *Con-*

II. *Concerning the Ratio between the Velocity of Windmill Sails unloaded, and their Velocity when loaded to a Maximum.*

Thoſe ratios, as they turned out in experiments upon different kinds of ſails, and with different inclinations (the velocity of the wind being the ſame) are contained in column 10 of Tab. III. where the extremes differ from the ratio of 10 : 7,7 to that of 10 : 5,8; but *the moſt general ratio of the whole will be nearly as* 3 : 2. This ratio alſo agrees ſufficiently near with experiments where the velocity of the wind was different, as in thoſe contained in Tab. IV. col. 13. in which the ratios differ from 10 : 6,9 to that of 10 : 5,9. However, it appears in general, that where the power is greater, whether by an enlargement of ſurface, or a greater velocity of the wind, that the ſecond term of the ratio is leſs.

III. *Concerning the Ratio between the greateſt Load that the Sails will bear without ſtopping, or what is nearly the ſame Thing, between the leaſt Load that will ſtop the Sails, and the Load at the Maximum.*

Thoſe ratios for different kinds of ſails and inclinations, are collected in col. 11. Tab. III. where the extremes differ from the ratio of 10 : 6 to that of 10 : 9,2; but taking in thoſe ſets of experiments only, where the ſails reſpectively anſwered beſt, *the ratios will be confined between that of* 10 : 8 *and of* 10 : 9; *and at a medium about* 10 : 8,3 *or of* 6 : 5. This ratio alſo agrees nearly with thoſe in col. 14. of Tab. IV. However it appears, upon the whole, that in thoſe inſtances, where the angle of the ſails or quantity of cloth were greateſt, that the ſecond term of the ratio was leſs.

IV. *Con-*

IV. *Concerning the Effects of Sails, according to the different Velocity of the Wind.*

Maxim 1. *The velocity of windmill sails, whether unloaded, or loaded, so as to produce a maximum, is nearly as the velocity of the wind, their shape and position being the same.*

This appears by comparing together the respective numbers of columns 4 and 5, Tab. IV. wherein those of numbers 2, 4 and 6, ought to be double of numbers 1, 3 and 5: but as the deviation is no where greater than what may be imputed to the inaccuracy of the experiments themselves, and hold good exactly in numbers 3 and 4; which sets were deduced from the medium of a number of experiments, carefully repeated the same day, and on that account are most to be depended upon; we may therefore conclude the maxim true.

Maxim 2d. *The load at the maximum is nearly, but somewhat less than, as the square of the velocity of the wind, the shape and position of the sails being the same.*

This appears by comparing together the numbers in col. 6. Tab. IV. wherein those of numbers 2, 4 and 6 (as the velocity is double), ought to be quadruple of those of numbers 1, 3 and 5; instead of which they fall short, number 2 by $\frac{1}{14}$, number 4 by $\frac{1}{19}$, and number 6 by $\frac{1}{13}$ part of the whole. The greatest of those deviations is not more considerable than might be imputed to the unavoidable errors in making the experiments: but as those experiments, as well as those of the greatest load, all deviate the same way; and also coincide with some experiments communicated to me by Mr. Rouse upon the resistance of planes; I am led to suppose a small deviation, whereby the load falls short of the squares of the velocity; and since the experiments N°. 3 and 4, are most to be depended upon, we must conclude,

 that

that when the velocity is double, the load falls ſhort of its due proportion by $\frac{1}{19}$, or, for the ſake of a round number, by about $\frac{1}{20}$ part of the whole.

Maxim 3d. *The effects of the ſame ſails at a maximum are nearly, but ſomewhat leſs than, as the cubes of the velocity of the wind.*

It has already been proved, Maxim 1ſt, that the velocity of ſails at the *maximum*, is nearly as the velocity of the wind; and by Maxim 2d, that the load at the *maximum* is nearly as the ſquare of the ſame velocity: if thoſe two maximums would hold preciſely, it would be a conſequence that the effect would be in a triplicate ratio thereof: how this agrees with experiment will appear by comparing together the products in col. 8 of Tab. IV. wherein thoſe of N°. 2, 4 and 6, (the velocity of the wind being double) ought to be octuple of thoſe of N°. 1, 3 and 5, inſtead of which they fall ſhort, N°. 2, by $\frac{1}{7}$, N°. 4, by $\frac{1}{20}$, and N°. 6, by $\frac{1}{6}$ part of the whole. Now, if we rely on No. 3 and 4, as the turns of the ſails are as the velocity of the wind; and ſince the load of the maximum falls ſhort of the ſquare of the velocity by about $\frac{1}{20}$ part of the whole: the product made by the multiplication of the turns into the load, muſt alſo fall ſhort of the triplicate ratio by about $\frac{1}{20}$ part of the whole product.

Maxim 4th. *The load of the ſame ſails at the maximum is nearly as the ſquares, and their effect as the cubes of their number of turns in a given time.*

This maxim may be eſteemed a conſequence of the three preceding; for if the turns of the ſails are as the velocity of the wind, whatever quantities are in any given ratio of the velocity of the wind will be in the ſame given ratio of the turns of the ſails: and therefore, if the load at the *maximum* is as the ſquare, or the effect as the cube, of the velocity of the wind, wanting $\frac{1}{20}$ part

part when the velocity is double; the load at the *maximum* will alſo be as the ſquare, and the effect as the cube, of the number of turns of the ſails in a given time, wanting in like manner $\frac{1}{10}$ part when the number of turns are double in the ſame time. In the preſent caſe, if we compare the loads at the *maximum*, col. 6, with the ſquares of the number of turns, col. 5, of N°. 1 and 2, 5 and 6, or the products of the ſame numbers col. 8, with the cubes of the number of turns col. 5, inſtead of falling ſhort, as N°. 3 and 4, they exceed thoſe ratios: but as the ſets of experiments No. 1 and 2, of 5 and 6, are not to be eſteemed of equal authority with thoſe of No. 3 and 4, we muſt not rely upon them further than to obſerve, that *in comparing the groſs effects of large machines, the direct proportion of the ſquares and cubes reſpectively, will hold as near as the effects themſelves can be obſerved*; and therefore be ſufficient for practical eſtimation, without any allowance.

Maxim 5th. *When ſails are loaded ſo as to produce a maximum at a given velocity, and the velocity of the wind increaſes, the load continuing the ſame; 1ſt, The increaſe of effect, when the increaſe of the velocity of the wind is ſmall, will be nearly as the ſquares of thoſe velocities: 2dly, When the velocity of the wind is double, the effects will be nearly as* 10 : $27\frac{1}{2}$: *But, 3dly, When the velocities compared, are more than double of that where the given load produces a maximum, the effects increaſe nearly in a ſimple ratio of the velocity of the wind.*

It has already been proved, maxim 1ſt and 2, that when the velocity of the wind is increaſed, the turns of the ſails will increaſe in the ſame proportion, even when oppoſed by a load as the ſquare of the velocity: and therefore if wanting the oppoſition of an increaſe of load, as the ſquare of the velocity, the turns of the ſails will again be increaſed in a ſimple ratio of the velocity of the wind on that account alſo; that is, the load continuing the ſame, the turns of the ſails in a given time will be as

 the

the ſquare of the velocity of the wind; and the effect, being in this caſe as the turns of the ſails will be as the ſquare of the velocity of the wind alſo; but this muſt be underſtood only of the firſt increments of the velocity of the wind: for,

2dly, As the ſails will never acquire above a given velocity in relation to the wind, though the load was diminiſhed to nothing; when the load continues the ſame, the more the velocity of the wind increaſes (though the effect will continue to increaſe) yet the more it will fall ſhort of the ſquare of the velocity of the wind; ſo that when the velocity of the wind is double, the increaſe of effect, inſtead of being as 1 : 4, according to the ſquares, it turns out as 10 : 27½, as thus appears. In Tab. IV. col. 9. the loads of N°. 2, 4 and 6, are the ſame as the maximum loads in col. 6, of N°. 1, 3 and 5. The number of turns of the ſails with thoſe loads, when the velocity of the wind is double, are ſet down in col. 10, and the products of their multiplication in col. 11: thoſe being compared with the products of N°. 1, 3 and 5, col. 8, furniſh the ratios ſet down in col. 12, which at a medium (due regard being had to N°. 3 and 4) will be nearly as 10 : 27½. 3dly. The load continuing the ſame, grows more and more inconſiderable, reſpecting the power of the wind as it increaſes in velocity; ſo that the turns of the ſails grow nearer and nearer a coincidence with their turns unloaded; that is, nearer and nearer to the ſimple ratio of the velocity of the wind. When the velocity of the wind is double, the turns of the ſails, when loaded to a maximum, will be double alſo; but, *unloaded*, will be no more than triple, by deduction 2d: and therefore the product could not have increaſed beyond the ratio of 10 : 30 (inſtead of 10 : 27½ even ſuppoſing the ſails not to have been retarded at all by carrying the maximum load for the half velocity. Hence we ſee, that when the velocity of the wind exceeds the double of that, where a conſtant load produces a maximum, that the increaſe of effect, which follows the increaſe of the velocity of the ſails, will be nearly as the velocity of the wind, and ultimately in that ratio preciſely.

Hence

Hence alſo we ſee that windmills, ſuch as the different ſpecies for raiſing water for drainage, &c. loſe much of their full effect, when acting againſt one invariable oppoſition.

V. *Concerning the Effects of Sails of different Magnitudes the Structure and Poſition being ſimilar, and the Velocity of the Wind the ſame.*

Maxim 6. *In ſails of a ſimilar figure and poſition, the number of turns in a given time will be reciprocally as the radius or length of the ſail.*

The extreme bar having the ſame inclination to the plane of its motion, and to the wind; its velocity at a *maximum* will always be in a given ratio to the velocity of the wind; and therefore, whatever be the radius, the abſolute velocity of the extremity of the ſail will be the ſame: and this will hold good reſpecting any other bar, whoſe inclination is the ſame, at a proportionable diſtance from the centre; it therefore follows, that the extremity of all ſimilar ſails, with the ſame wind, will have the ſame abſolute velocity; and therefore take a ſpace of time to perform one revolution in proportion to the radius; or, which is the ſame thing, the number of revolutions in the ſame given time, will be reciprocally as the length of the ſail.

Maxim 7. *The load at a maximum that ſails of a ſimilar figure and poſition will overcome, at a given diſtance from the centre of motion, will be as the cube of the radius.*

Geometry informs us, that in ſimilar figures the ſurfaces are as the ſquares of their ſimilar ſides; of conſequence the quantity of cloth will be as the ſquare of the radius: alſo in ſimilar figures and poſitions, the impulſe of the wind, upon every ſimilar ſection of the cloth, will be in proportion to the ſurface

 of

of that ſection; and conſequently, the impulſe of the wind upon the whole, will be as the ſurface of the whole: but as the diſtance of every ſimilar ſection, from the centre of motion, will be as the radius; the diſtance of the centre of power of the whole, from the centre of motion, will be as the radius alſo; that is, the lever by which the power acts, will be as the radius: as therefore the impulſe of the wind, reſpecting the quantity of cloth, is as the ſquare of the radius, and the lever, by which it acts, as the radius ſimply; it follows, that the load which the ſails will overcome, at a given diſtance from the centre, will be as the cube of the radius.

Maxim 8. *The effect of ſails of ſimilar figure and poſition, are as the ſquare of the radius.*

By maxim 6, it is proved, that the number of revolutions made in a given time, are as the radius inverſely. Under maxim 7, it appears, that the length of the lever, by which the power acts, is as the radius directly; therefore theſe equal and oppoſite ratios deſtroy one another: but as in ſimilar figures the quantity of cloth is as the ſquare of the radius, and the action of the wind is in proportion to the quantity of cloth, as alſo appears under maxim 7; it follows that the effect is as the ſquare of the radius.

Corol. 1. Hence it follows, that augmenting the length of the ſail, without augmenting the quantity of cloth, does not increaſe the power; but becauſe what is gained by the length of the lever, is loſt by the ſlowneſs of the rotation.

Corol. 2. If ſails are increaſed in length, the breadth remaining the ſame, the effect will be as the radius.

VI. *Con-*

VI. *Concerning the Velocity of the Extremities of Wind-mill Sails, in Respect to the Velocity of the Wind.*

Maxim 9. *The velocity of the extremities of* Dutch *sails, as well as of the enlarged sails, in all their usual positions when unloaded, or even loaded to a maximum, are considerably quicker than the velocity of the wind.*

The *Dutch* sails unloaded, as in Tab. III. N° 8. made 120 revolutions in 52″: the diameter of the sails being 3 feet 6 inches, the velocity of their extremities will be 25,4 feet in a second; but the velocity of the wind producing it, being 6 feet in the same time, we shall have 6 : 25,4 : : 1 : 4,2; in this case therefore, the velocity of their extremities was 4,2 times greater than that of the wind. In like manner, the relative velocity of the wind, to the extremities of the same sails, when loaded to a *maximum*, making then 93 turns in 52″, will be found to be as 1 : 3,3; or 3,3 times quicker than that of the wind.

The following table contains 6 examples of *Dutch* sails, and 4 examples of the enlarged sails, in different positions, but with the constant velocity of the wind of 6 feet in a second, from Table III. and also 6 examples of *Dutch* sails in different positions, with different velocities of the wind from Table IV.

TABLE

TABLE V. *Containing the Ratio of the Velocity of the Extremities of Windmill Sails to the Velocity of the Wind.*

N°.	N°. of Tab. III. and IV.	Angle at the Extremity.	Velocity of the wind in a second.	Ratio of the velocity of the wind and extremities of the sails, unloaded.	loaded.	
1	8	0°	6f. 0in.	1 : 4,2	1 : 3,3	From Table III.
2	9	3	6 0	1 : 4,2	1 : 2,8	
3	10	5	6 0	— —	1 : 2,75	
4	11	7	6 0	1 : 4,	1 : 2,7	
5	12	10	6 0	1 : 3,8	1 : 2,6	
6	13	12	6 0	1 : 3,5	1 : 2,3	
7	14	7½	6 0	1 : 4,3	1 : 2,6	
8	15	10	6 0	1 : 4,1	1 : 2,6	
9	16	12	6 0	1 : 4,	1 : 2,3	
10	17	15	6 0	1 : 3,35	1 : 2,2	
11	1	5	4 4½	1 : 4,	1 : 2,8	From Tab. IV.
12	2	5	8 9	1 : 4,3	1 : 2,6	
13	3	7½	4 4½	— —	1 : 2,8	
14	4	7½	8 9	— —	1 : 2,7	
15	5	10	4 4½	1 : 3,8	1 : 2,6	
16	6	10	8 9	1 : 3,4	1 : 23	
1	2	3	4	5	6	

It appears from the preceding collection of examples, that when the extremities of the *Dutch* sails are parallel to the plane of motion, or at right angles to the wind, and to the axis, as they are made according to the common practice in *England*, that their velocity, unloaded, is above 4 times, and loaded to a *maximum*, above 3 times greater than that of the wind: but that when the Dutch sails, or enlarged sails, are in their best positions, their velocity

velocity unloaded is 4 times, and loaded to a *maximum*, at a medium the Dutch ſails are 2,7, and the enlarged ſails 2,6 times greater than the velocity of the wind. Hence we are furniſhed with a method of knowing the velocity of the wind, from obſerving the velocity of the windmill ſails; for knowing the radius, and the number of turns in a minute, we ſhall have the velocity of the extremities; which, divided by the following diviſors, will give the velocity of the wind.

Dutch ſails in the common poſition	unloaded	4.2
	loaded	—3.3
Dutch ſails in their beſt poſition -	unloaded	4.0
	loaded	—2.7
Enlarged ſails in the beſt poſition -	unloaded	4.0
	loaded	—2.6

From the above diviſors there ariſes the following compendiums; ſuppoſing the radius to be 30 feet, which is the moſt uſual length in this country, and the mill to be loaded to a *maximum*, as is uſually the caſe with corn mills; for every 3 *turns in a minute, of the Dutch ſails in their common poſition, the wind will move at the rate of 2 miles an hour*; for every 5 *turns in a minute, of the Dutch ſails in their beſt poſition, the wind moves* 4 *miles an hour*; and for every 6 *turns in a minute, of the enlarged ſails in their beſt poſition, the wind will move* 5 *miles an hour.*

The following table, which was communicated to me by my friend Mr. Rouſe, and which appears to have been conſtructed with great care, from a conſiderable number of facts and experiments, and which having relation to the ſubject of this article; I here inſert it as he ſent it to me: but at the ſame time muſt obſerve, that the evidence for thoſe numbers where the velocity of the wind exceeds 50 miles an hour, do not ſeem of equal authority with thoſe of 50 miles an hour and under. It is alſo to be

be obſerved, that the numbers in col. 3. are calculated according to the ſquare of the velocity of the wind, which, in moderate velocities, from what has been before obſerved, will hold very nearly.

TABLE VI. *Containing the Velocity and Force of Wind, according to their common Appellations.*

Velocity of the Wind.		Perpendicular force on one foot area in pounds avoirdupois.	Common appellations of the force of winds.
Miles in one Hour.	Feet in one ſecond.		
1	1,47	,005	Hardly perceptible.
2	2,93	,020	Juſt perceptible.
3	4,40	,044	
4	5,87	,079	Gentle pleaſant wind.
5	7,33	,123	
10	14,67	,492	Pleaſant briſk gale.
15	22,00	1,107	
20	29,34	1,968	Very briſk.
25	36,67	3,075	
30	44,01	4,429	High winds.
35	51,34	6,027	
40	58,68	7,873	Very high.
45	66,01	9,963	
50	73,35	12,300	A ſtorm or tempeſt.
60	88,02	17,715	A great ſtorm.
80	117,36	31,490	An hurricane.
100	146,70	49,200	An hurricane that tears up trees, carries buildings before it, &c.
1	2	3	

VII.

VII. *Concerning the absolute Effect, produced by a given Velocity of the Wind, upon Sails of a given Magnitude and Construction.*

It has been observed by practitioners, that in mills with Dutch sails in the common position, that when they make about 13 turns in a minute, they then work at a mean rate: that is, by the compendiums in the last article, when the velocity of the wind is $8\frac{2}{3}$ miles an hour, or $12\frac{2}{3}$ feet in a second; which, in common phrase, would be called a *fresh gale.*

The experiments set down in Tab. IV. N° 4. were tried with a wind, whose velocity was $8\frac{3}{4}$ feet in a second; consequently had those experiments been tried with a wind, whose velocity was $12\frac{2}{3}$ feet in a second, the effect, by maxim 3d, would have been 3 times greater; because the cube of $12\frac{2}{3}$ is 3 times greater than that of $8\frac{3}{4}$.

From Tab. IV. N° 4. we find that the sails, when the velocity of the wind was $8\frac{3}{4}$ feet in a second, made 130 revolutions in a minute, with a load of 17,52 lb. From the measures of the machine, preceding the specimen of a set of experiments, we find, that 20 revolutions of the sails raised the scale and weight 11,3 inches: 130 revolutions will therefore raise the scale 73,45 inches, which, multiplied by 17,52 lb. makes a product of 1287, for the effect of the Dutch sails in their best position; that is, when the velocity of the wind is $8\frac{3}{4}$ feet in a second: this product therefore multiplied by three, will give 3861 for the effect of the same sails, when the velocity of the wind is $12\frac{2}{3}$ feet in a second.

Desaguliers makes the utmost power of a man, when working so as to be able to hold it for some hours, to be equal to that of

of raiſing an hogſhead of water 10 feet high in a minute. Now, an hogſhead conſiſting of 63 ale gallons, being reduced into pounds avoirdupois, and the height into inches; the product made by multiplying thoſe two numbers will be 76800; which is 19 times greater than the product of the ſails laſt-mentioned, at $12\frac{1}{2}$ feet in a ſecond: therefore, by maxim 8th, if we multiply the ſquare root of 19, that is 4,46, by 21 inches, the length of the ſail producing the effect 3861, we ſhall have 93,66 inches, or 7 feet $9\frac{2}{3}$ inches for the radius of a Dutch ſail in its beſt poſition, whoſe mean power ſhall be equal to that of a man: but if they are in their common poſition, their length muſt be increaſed in the ratio of the ſquare root of 442 to that of 639, as thus appears;

The ratio of the *maximum* products of N°. 8 and 11. Tab. III. are as 442 : 639: but by maxim 8, the effects of ſails of different radii are as the ſquare of the radii; conſequently the ſquare roots of the products or effects, are as the radii ſimply; and therefore as the ſquare root of 442 is to that of 639; ſo is 93,66 to 112,66; or 9 feet $4\frac{2}{3}$ inches.

If the ſails are of the enlarged kind, then from Tab. III. N°. 11 and 15. we ſhall have the ſquare root of 820 to that of 639 : : 93,66 : 82,8 inches, or 6 feet $10\frac{3}{4}$ inches: ſo that in round numbers we ſhall have the radius of a ſail, of ſimilar figure to their reſpective models, whoſe mean power ſhall be equal to that of a man;

The Dutch ſails in their common poſition	$9\frac{1}{2}$ feet.
The Dutch ſails in their beſt poſition ——	8
The enlarged ſails in their beſt poſition —	7

Suppoſe now the radius of a ſail to be 30 feet, and to be conſtructed upon the model of the enlarged ſails, N°. 14 or 15. Tab. III. dividing 30 by 7 we ſhall have 4,28, the ſquare of which is

is 18,3; and this, according to maxim 7, will be the relative power of a ſail of 30 feet, to one of 7 feet; that is, when working at a mean rate, the 30 feet ſail will be equal to the power of 18,3 men, or of $3\frac{2}{3}$ horſes; reckoning 5 men to a horſe: whereas the effect of the common Dutch ſails, of the ſame length, being leſs in the proportion of 820 : 442, will be ſcarce equal to the power of 10 men, or of 2 horſes.

That theſe computations are not merely ſpeculative, but will nearly hold good when applied to works in large, I have had an opportunity of verifying: for in a mill with the enlarged ſails of 30 feet applied to the cruſhing of rape ſeed, by means of two runners, upon the edge, for making oil; I obſerved, that when the ſails made 11 turns in a minute, in which caſe the velocity of the wind was about 13 feet in a ſecond, according to article 6th, that the runners then made 7 turns in a minute: whereas 2 horſes, applied to the ſame 2 runners, ſcarcely worked them at the rate of $3\frac{1}{2}$ turns in the ſame time. Laſtly, with regard to the real ſuperiority of the enlarged ſails, above the Dutch ſails as commonly made, it has ſufficiently appeared, not only in thoſe caſes where they have been applied to new mills, but where they have been ſubſtituted in the place of the others.

VIII. *Concerning horizontal Windmills and Water-Wheels, with oblique Vanes.*

Obſervations upon the effects of common windmills, with oblique vanes, have led many to imagine, that could the vanes be brought to receive the direct impulſe, like a ſhip ſailing before the wind, it would be a very great improvement in point of power: while others attending to the extraordinary and even unexpected effects of oblique vanes have been led to imagine that oblique vanes applied to water-mills, would as much exceed the common water-wheels, as the vertical wind-mills are found to have exceeded all attempts towards an horizontal one. Both theſe notions,

notions, but especially the first, have so plausible an appearance, that of late years there has seldom been wanting those, who have assiduously employed themselves to bring to bear designs of this kind: it may not therefore be unacceptable to endeavour to set this matter in a clear light.

PLATE III. fig. 2. Let A B be the section of a plane, upon which let the wind blow in the direction C D, with such a velocity as to describe a given space B E, in a given time (suppose 1 second); and let A B be moved parallel to itself, in the direction C D. Now, if the plane A B moves with the same velocity as the wind; that is, if the point B moves through the space B E in the same time that a particle of air would move through the same space; it is plain that, in this case, there can be no pressure or impulse of the wind upon the plane: but if the plane moves slower than the wind, in the same direction, so that the point B may move to F, while a particle of air, setting out from B at the same instant, would move to E, then B F will express the velocity of the plane; and the relative velocity of the wind and plane will be expressed by the line F E. Let the ratio of F E to B E be given (suppose 2 : 3); let the line A B represent the impulse of the wind upon the plane A B, when acting with its whole velocity B E; but, when acting with its relative velocity, F E, let its impulse be denoted by some aliquot part of A B, as for instance $\frac{4}{9}$ A B: then will $\frac{4}{9}$ of the parallelogram A F represent the mechanical power of the plane; that is, $\frac{4}{9}$ A B $\times \frac{1}{3}$ B E.

2dly, Let I N be the section of a plane, inclined in such a manner, that the base I K of the rectangle triangle I K N may be equal to A B; and the perpendicular N K=B E; let the plane I N be struck by the wind, in the direction L M, perpendicular to I K: then, according to the known rules of oblique forces, the impulse of the wind upon the plane I N, tending to move it according to the direction L M, or N K, will be

be denoted by the baſe I K; and that part of the impulſe, tending to move it according to the direction I K, will be expreſſed by the perpendicular N K. Let the plane I N be moveable in the direction of I K only; that is, the point I in the direction of I K, and the point N in the direction N Q, parallel thereto. Now it is evident, that if the point I moves through the line I K, while a particle of air, ſetting forwards at the ſame time from the point N, moves through the line N K, they will both arrive at the point K at the ſame time; and conſequently, in this caſe alſo, there can be no preſſure or impulſe of the particle of the air upon the plane I N. Now let I O be to I K as B F to B E; and let the plane I N move at ſuch a rate, that the point I may arrive at O, and acquire the poſition I Q, in the ſame time that a particle of wind would move through the ſpace N K: as O Q is parallel to I N; (by the properties of ſimilar triangles) it will cut N K in the point P, in ſuch a manner, that N P = B F, and P K = F E: hence it appears, that the plane I N, by acquiring the poſition O Q, withdraws itſelf from the action of the wind, by the ſame ſpace N P, that the plane A B does by acquiring the poſition F G; and conſequently, from the equality of P K to F E, the relative impulſe of the Wind P K, upon the plane O Q, will be equal to the relative impulſe of the wind F E, upon the plane F G: and ſince the impulſe of the wind upon A B, with the relative velocity F E, in the direction B E, is repreſented by $\frac{4}{9}$ A B; the relative impulſe of the wind upon the plane I N, in the direction N K, will in like manner be repreſented by $\frac{4}{9}$ I K; and the impulſe of the wind upon the plane I N, with the relative velocity P K, in the direction I K, will be repreſented by $\frac{4}{9}$ N K: and conſequently the mechanical power of the plane I N, in the direction I K, will be $\frac{4}{9}$ the parallelogram I Q: that is $\frac{1}{3}$ I K $\times$ $\frac{4}{9}$ N K: that is, from the equality of I K = A B and N K = B E, we ſhall have $\frac{4}{9}$ I Q = $\frac{1}{3}$ A B $\times$ $\frac{4}{9}$ B E = $\frac{4}{9}$ A B $\times$ $\frac{1}{3}$ B E = $\frac{4}{9}$ of the area of the parallelogram A F. Hence we deduce this

F GENERAL

General Proposition,

That all planes, however ſituated, that intercept the ſame ſection of the wind, and having the ſame relative velocity, in regard to the wind, when reduced into the ſame direction, have equal powers to produce mechanical effects.

For what is loſt by the obliquity of the impulſe, is gained by the velocity of the motion.

Hence it appears, that an oblique ſail is under no diſadvantage in reſpect of power, compared with a direct one; except what ariſes from a diminution of its breadth, in reſpect to the ſection of the wind: the breadth I N being by obliquity reduced to I K.

The diſadvantage of horizontal windmills therefore does not conſiſt in this; that each ſail, when directly expoſed to the wind is capable of a leſs power, than an oblique one of the ſame dimenſions; but that in an horizontal windmill, little more than one ſail can be acting at once: whereas in the common windmill, all the four act together: and therefore, ſuppoſing each vane of an horizontal windmill, of the ſame dimenſions as each vane of the vertical, it is manifeſt the power of a vertical mill with four ſails, will be four times greater than the power of the horizontal one, let its number of vanes be what it will: this diſadvantage ariſes from the nature of the thing; but if we conſider the further diſadvantage, that ariſes from the difficulty of getting the ſails back again againſt the wind, &c. we need not wonder if this kind of mill is in reality found to have not above $\frac{1}{8}$ or $\frac{1}{10}$ of the power of the common ſort; as has appeared in ſome attempts of this kind.

In

In like manner, as little improvement is to be expected from water-mills with oblique vanes: for the power of the same section of a stream of water, is not greater when acting upon an oblique vane, than when acting upon a direct one: and any advantage that can be made by intercepting a greater section, which sometimes may be done in the case of an open river, will be counterbalanced by the superior resistance, that such vanes would meet with by moving at right angles to the current: whereas the common floats always move with the water nearly in the same direction.

Here it may reasonably be asked, that since our geometrical demonstration is general, and proves, that one angle of obliquity is as good as another, why in our experiments it appears, that there is a certain angle which is to be preferred to all the rest? It is to be observed, that if the breadth of the sail IN is given, the greater the angle KIN, and the less will be the base IK: that is, the section of wind intersected, will be less: on the other hand, the more acute the angle KIN, the less will be the perpendicular KN: that is, the impulse of the wind, in the direction IK being less, and the velocity of the sail greater; the resistance of the medium will be greater also. Hence therefore, as there is a diminution of the section of the wind intercepted on one hand, and an increase of resistance on the other, there is some angle, where the disadvantage arising from these causes upon the whole is the least of all; but as the disadvantage arising from resistance is more of a physical than geometrical consideration, the true angle will best be assigned by experiments.

Scholium.

In trying the experiments contained in Tab. III. and IV. the different specific gravity of the air, which is undoubtedly different at different times, will cause a difference in the load, proportional to the difference of its specific gravity, though its

 velocity

velocity remains the ſame; and a variation of ſpecific gravity may ariſe not only from a variation of the weight of the whole column, but alſo by the difference of heat of the air concerned in the experiment, and poſſibly of other cauſes; yet the irregularities that might ariſe from a difference of ſpecific gravity were thought to be too ſmall to be perceivable, till after the principal experiments were made, and their effects compared; from which, as well as ſucceeding experiments, thoſe variations were found to be capable of producing a ſenſible, though no very conſiderable effect: however, as all the experiments were tried in the ſummer ſeaſon, in the day-time, and under cover, we may ſuppoſe that the principal ſource of error would ariſe from the different weight of the column of the atmoſphere at different times: but as this ſeldom varies above $\frac{1}{15}$ part of the whole, we may conclude, that though many of the irregularities contained in the experiments referred to in the foregoing eſſay, might ariſe from this cauſe; yet as all the principal concluſions are drawn from the medium of a conſiderable number, many whereof were made at different times, it is preſumed that they will nearly agree with the truth, and be altogether ſufficient for regulating the practical conſtruction of thoſe kind of machines, for which uſe they were principally intended.

AN

EXPERIMENTAL EXAMINATION

OF THE QUANTITY AND PROPORTION OF

MECHANIC POWER

NECESSARY TO BE EMPLOYED IN GIVING DIFFERENT DEGREES OF VELOCITY TO

HEAVY BODIES

FROM A

STATE OF REST.

By Mr. JOHN SMEATON, F. R. S.

AN

EXPERIMENTAL EXAMINATION, &c.

Read before the Royal Society, April 25, 1776.

ABOUT the year 1686 Sir ISAAC NEWTON first published his *Principia*, and, conformably to the language of mathematicians of those times defined, that "the quantity of "motion is the measure of the same, arising from the velocity "and quantity of matter conjointly." Very soon after this publication, the truth or propriety of this definition was disputed by certain philosophers, who contended, that the measure of the quantity of motion should be estimated by taking the quantity of matter and the square of the velocity conjointly. There is nothing more certain, than that from equal impelling powers, acting for equal intervals of time, equal increases of velocity are acquired by given bodies, when unresisted by a medium; thus gravity causes a body, in obeying its impulse during one second of time, to acquire a velocity which would carry it uniformly forward, without any additional impulse, at the rate of 32 ft. 2 in. *per* second; and if gravity is suffered to act

upon it for two feconds, it will have, in that time, acquired a velocity that would carry it, at an uniform rate, juft double of the former; that is, at the rate of 64 ft. 4 in. *per* fecond. Now, if in confequence of this equal increafe of velocity, in an equal increafe of time, by the continuance of the fame impelling power, we define that to be a double quantity of motion, which is generated in a given quantity of matter, by the action of the fame impelling power for a double time; this will be co-incident with Sir ISAAC NEWTON's definition above mentioned; whereas, in trying experiments upon the total effects of bodies in motion, it appears, that when a body is put in motion, by whatever caufe, the impreffion it will make upon an uniformly refifting medium, or upon uniformly yielding fubftances, will be as the mafs of matter of the moving body, multiplied by the fquare of its velocity: the queftion, therefore, properly is, whether thofe terms, the *quantity of motion*, the *momenta* of bodies in motion, or *forces* of bodies in motion, which have generally been efteemed fynonymous, are with the moft propriety of language to be efteemed equal, double, or triple, when they have been generated by an equable impulfe, acting for an equal, double, or triple time; or that it fhould be meafured by the effects being equal, double, or triple, in overcoming refiftances before a body in motion can be ftopped? For, according as thofe terms are underftood in this or that way, it will neceffarily follow, that the *momenta* of equal bodies will be as the velocities, or as the fquares of the velocities, or as the fquares of the velocities refpectively; it being certain, that, whichever we take for the proper definition of the term quantity of motion, by paying a proper regard to the collateral circumftances that attend the application of it, the fame conclufion, in point of computation, will refult. I fhould not, therefore, have thought it worth while to trouble the Society upon this fubject, had I not found, that not only myfelf and other practical artifts, but alfo fome of the moft approved writers, had been liable to fall into errors, in applying thefe doctrines to practical mechanics, by fometimes forgetting or neglecting

neglecting the due regard which ought to be had to these collateral circumstances. Some of these errors are not only very considerable in themselves, but also of great consequence to the public, as they tend greatly to mislead the practical artist in works that occur daily, and which often require very great sums of money in their execution. I shall mention the following instances.

Desaguliers, in his second volume of Experimental Philosophy, treating upon the question concerning the forces of bodies in motion, after taking much pains to shew that the dispute, which had then subsisted fifty years, was a dispute about the meaning of words; and that the same conclusion will be brought out, when things are rightly understood, either upon the old or new opinion, as he distinguishes them; among other things, tells us, that the old and new opinion may be easily reconciled in this instance: that the wheel of an undershot water-mill is capable of doing quadruple work when the velocity of the water is doubled, instead of double work only; "because (the adjutage being the same), says he, we find, that "as the water's velocity is double, there are twice the number "of particles of water that issue out, and therefore the ladle-"board is struck by twice the matter, which matter moving "with twice the velocity that it had in the first case, the whole "effect must be quadruple, though the instantaneous stroke of "each particle is increased only in a simple proportion of the "velocity." See vol. II. Annotations on lecture 6th, p. 92.

Again, in the same volume, lecture 12th, p. 424, referring to what went before, he tells us, "The knowledge of the fore-"going particulars is absolutely necessary for setting an under-"shot wheel to work; but the advantage to be reaped from it "would be still guesswork, and we should be still at a loss to "find out the utmost it can perform, if we had not an in-"genious proposition of that excellent mechanic M. Parent,

"of

" of the Royal Academy of Sciences, who has given us a " *maximum* in this cafe, by fhewing, that an underfhot wheel " can do the moft work, when its velocity is equal to the " third part of the velocity of the water that drives it, &c. " becaufe then two-thirds of the water is employed in driving " the wheel with a force proportionable to the fquare of its " velocity. If we multiply the furface of the adjutage or open- " ing by the height of the water, we fhall have the column of " water that moves the wheel. The wheel thus moved will " fuftain on the oppofite fide only four-ninths of that weight, " which will keep it in equilibrio; but what it can move with " the velocity it goes with, will be but one-third of that weight " of equilibrium; that is, $\frac{4}{27}$ths of the weight of the firft " column, &c.—This is the utmoft that can be expected."

The fame conclufion is likewife adopted by MACLAURIN, in art. 907. p. 728. of his Fluxions, where, giving the fluxionary deduction of M. PARENT's propofition, he fays, " that if A re- " prefents the weight which would balance the force of the " ftream, when its velocity is a; and U reprefents the velocity " of the part of the engine, which it ftrikes when the motion " of the machine is uniform, &c.—the machine will have the " greateft effect when U is equal to $\frac{a}{3}$; that is, if the weight " that is raifed by the engine be lefs than the weight which " would balance the power, in the proportion of 4 to 9, and the " *momentum* of the weight is $\frac{4Aa}{27}$."

Finding that thefe conclufions were far from the truth; and feeing, from many other circumftances, that the practical theory of making water and wind-mills was but very imperfectly delivered by any author I had then an opportunity of confulting*; in

* BELIDOR, *Architecture Hydraulique*, greatly prefers the application of water to an underfhot mill, inftead of an overfhot; and attempts

in the year 1751 I began a courſe of experiments upon this ſubject. Theſe experiments, with the concluſions drawn from them, have already been communicated to this Society, who printed them in vol. LI. of their Tranſactions for the year 1759, and for this communication I had the honour of receiving the annual medal of Sir GODFREY COPLEY, from the hands of our very worthy Preſident the late Earl of MACCLESFIELD. Thoſe experiments and concluſions ſtand uncontroverted, ſo far as I know, to this day; and having ſince that time been concerned in directing the conſtruction of a great number of mills, which were all executed upon the principles deduced from them, I have by that means had many opportunities of comparing the effects actually produced with the effects which might be expected from the calculation; and the agreement, I have always found between theſe two, appears to me fully to eſtabliſh the truth of the

attempts to demonſtrate, that water applied underſhot will do ſix times more execution than the ſame applied overſhot. See vol. I. p. 286. While DESAGULIERS, endeavouring to invalidate what had been advanced by BELIDOR, and greatly preferring an overſhot to an underſhot, ſays, Annotat. on lecture 12. vol. II. p. 532. that from his own experience, "a well-made overſhot mill ground "as much corn in the ſame time with ten times leſs water;" ſo that betwixt BELIDOR and DESAGULIERS, here is a difference of no leſs than 60 to 1.

Again, BELIDOR, vol. II. p. 72. ſays, that the centre of gravity of each ſail of a windmill ſhould travel in its own circle with one-third of the velocity of the wind; ſo that, taking the diſtance of this centre of gravity from the centre of motion at 20 feet, as he ſtates it p. 38. art. 849. the circumference will be exceeding 126 feet Engliſh meaſure: a wind, therefore, to make the mill go twenty turns *per* minute, which they frequently do with a freſh wind and all their cloth ſpread, would require the wind to move above eighty miles an hour; a velocity perhaps hardly equalled in the greateſt ſtorms we experience in this climate.

princi-

principles upon which they were constructed, when applied to great works, as well as upon a smaller scale in models.

Respecting the explanatory deduction of DESAGULIERS in the first example abovementioned, which, indeed, I have found to be the commonly received doctrine among theoretical mechanics, it is shewn, in my former Essay, page 21, 22, and 24, part 1, maxim 4, that, where the velocity of water is double, the adjutage or aperture of the sluice remaining the same, the effect is eight times; that is, not as the square but as the cube of the velocity; and the same is investigated concerning the power of the wind arising from difference of velocity, p. 52, being part 3, maxim 4.

The conclusion in the second example abovementioned, adopted both by DESAGULIERS and MACLAURIN, is not less wide of the truth than the foregoing; for if that conclusion were true, only $\frac{4}{27}$ths of the water expended could be raised back again to the height of the reservoir from which it had descended, exclusively of all kinds of friction, &c. which would make the actual quantity raised back again still less; that is, less than one-seventh of the whole; whereas it appears, from Table I. of the preceding essay, that in some of the experiments there related, even upon the small scale on which they were tried, the work done was equivalent to raising back again about one quarter of the water expended; and in large works the effect is still greater, approaching towards half, which seems to be the limit for the undershot mills, as the whole would be the limit for the overshot mills, if it were possible to set aside all friction, resistance from the air, &c. see p. 29.

The velocity also of the wheel, which, according to M. PARENT's determination, adopted by DESAGULIERS and MACLAURIN, ought to be no more than one-third of that of the water, varies at the *maximum* in the abovementioned experiments of

Table

Table I. between one-third and one-half; but in all the cafes there related, in which the moft work is performed in proportion to the water expended, and which approach the neareft to the circumftances of great works, when properly executed, the *maximum* lies much nearer to one-half than one-third; one-half feeming to be the true *maximum*, if nothing were loft by the refiftance of the air, the fcattering of the water carried up by the wheel, and thrown off by the centrifugal force, &c. all which tend to diminifh the effect more, at what would be the *maximum* if thefe did not take place, than they do when the motion is a little flower.

Finding thefe matters, as well as others, to come out in the experiments, fo very different from the opinions and calculations of authors of the firft reputation, who, reafoning according to the Newtonian definition, muft have been led into thefe errors from a want of attending to the proper collateral circumftances; I thought it very material, efpecially for the practical artift, that he fhould make ufe of a kind of reafoning in which he fhould not be fo liable to miftakes; in order, therefore, to make this matter perfectly clear to myfelf, and poffibly fo to others, I refolved to try a fet of experiments from whence it might be inferred, what proportion or quantity of mechanical power is expended in giving the fame body different degrees of velocity. This fcheme was put in execution in the year 1759, and the experiments were then fhewn to feveral friends, particularly my very worthy and ingenious friend Mr. William Russell.

In my experimental inquiry concerning the powers of water and wind before referred to, I have, p. 105, part 1. defined what I meant by power as applied to practical mechanics, that is, what I now call mechanical power; which, in terms fynonymous to thofe there ufed, may be faid to be meafured by multiplying the weight of the body into the perpendicular height from which

which it can defcend; thus the fame weight, defcending from a double height, is capable of producing a double mechanical effect, and is therefore a double mechanical power. A double weight defcending from the fame height is alfo a double power, becaufe it likewife is capable of producing a double effect; and a given body, defcending through a given perpendicular height, is the fame power as a double body defcending through half that perpendicular; for, by the intervention of proper levers, they will counterbalance one another, conformably to the known laws of mechanics, which have never been controverted. It muft, however, be always underftood, that the defcending body, when acting as a meafure of power, is fuppofed to defcend flowly, like the weight of a clock or a jack; for, if quickly defcending, it is fenfibly compounded with another law, *viz.* the law of acceleration by gravity.

DESCRIPTION OF THE MACHINE.

A B is the bafe of the machine placed upon a table.

A C is a pillar or ftandard.

C D is an arm, upon the extremity of which is fixed a plate *f g*, which is here feen edge-ways, through which is a fmall hole for receiving a fmall fteel pivot *e*, fixed in the top of the upright axis *e* B; the lower end of this axis finifhes in a conical fteel point, which refts upon a fmall cup of hard fteel polifhed at B.

H I is a cylinder of white fir, which paffes through a perforation in the axis, and therein fixes; and, upon the two arms formed thereby, are capable of fliding.

K L two cylindric weights of lead of equal fize, which are capable of being fixed upon any part of the cylindric arms, from

from the axis to their extremities, by means of two thin wedges of wood. The two weights, therefore, being at equal diſtances from the centre, and the axis perpendicular, the whole will be balanced upon the point at B, and moveable thereupon by an impelling power, with very little friction.

Upon the upper part of the axis are formed M N, two cylindrical barrels, whereof M is double the diameter of N; they have a little pin ſtuck into one ſide of each at *o*, *p*.

Q is a piece capable of ſliding higher or lower, as occaſion requires; and carries

R, a light pulley of about three inches diameter, hung upon a ſteel axis, and moveable upon two ſmall pivots. The plane of the pulley, however, is not directed to the middle of the upright axis, but a little on one ſide, ſo as to point (at a mean) between the ſurface of the bigger barrel and the leſs.

S is a light ſcale for receiving weights, and hangs by a ſmall twine, cord, or line, that paſſes the pulley, and terminates either upon the bigger barrel or the leſs, as may be required; the ſliding-piece Q being moved higher or lower for each, that the line, in paſſing from the pulley to the barrel, may be nearly horizontal. The end of the line, that is furtheſt from the ſcale, is terminated by a ſmall loop, which hangs on upon the pin *o*, or the pin *p*, according as the bigger or the leſſer barrel is to be uſed.

Now, having wound up a certain number of turns of the line upon the barrel, and having placed a weight in the ſcale S, it is obvious, that it will cauſe the axis to turn round, and give motion to its arms, and to the weights of lead placed thereon, which are the heavy bodies to be put in motion by the impulſe of the weight in the ſcale; and when the line is wound off to the pin,

pin, the loop slips off, and the scale then falling down, the weight will cease to accelerate the motion of the heavy bodies, and leave them revolving, equably forward, with the velocity they have acquired, except so far as it must be gradually lessened by the friction of the machine and resistance of the air, which being small, the bodies will revolve sometime before their velocity is apparently diminished.

MEASURES OF SOME PARTS OF THE MACHINE.

	Inches.
Diameter of the cylinders of lead, or the heavy bodies	2,57
Length of ditto - - - - - - - - - - - - - -	1,56
Diameter of the hole therein - - - - - - - - -	,72

Weight of each cylinder 3 lbs. Avoirdupois.

Greater distance of the middle of each body from the centre of the axis - - - - - - - - -	8,25
The smaller distance of ditto - - - - - - - -	3,92
10 turns of the smaller barrel raises the scale, 5 ditto of the bigger ditto - - - - - - - - -	25,25

When the bodies are at the smaller distance above specified from the axis of rotation, they are then in effect at half the greater distance from that axis: for, since the axis itself, and the cylindric arms of wood, keep an unvaried distance from the centre of rotation, the bodies themselves must be moved nearer than half their former distance, in order that, compounded with the unvariable parts, they may be virtually at the half distance. In order to find this half distance nearly, I put in an arm of the same wood, that only went through the axis, without extending in the opposite direction; one of the bodies being put upon the end of this arm, at the distance of 8,25 inches, the whole machine was inclined till the body and arm became a kind of pen-

dulum,

dulum, and vibrated at the rate of 92 times *per* minute; and as a pendulum of the half length vibrates quicker in the proportion of $\surd$ 1 to $\surd$ 2; that is, in the proportion of 92 to 130 nearly; therefore, keeping the same inclination of the machine, the weight was moved upon the arm till it made 130 vibrations *per* minute; which was found to be, when it was at 3,92 inches distance from the centre as above stated, which is about $\frac{2}{10}$ths of an inch nearer than the half distance. The double arm was then put in, and marked accordingly, and the bodies being mounted thereon, the whole was adjusted ready for use; and with it were tried the following experiments, each of which was repeated so many times as to be fully satisfactory.

TABLE OF EXPERIMENTS.

N°.	Ounces Avoirdupois in the Scale.	Barrel used, M the bigger, N the smaller.	The Arms, W the whole, H the half-length.	Number of Turns of the Line wound on the Barrel.	Time of the Descent of the Weight in the Scale.	Time in making 20 Revolutions with equal motions.
					''	''
1	8	M	W	5	$14\frac{1}{4}$	29
2	8	N	W	10	$28\frac{1}{4}$	$29\frac{1}{4}$
3	8	N	W	$2\frac{1}{2}$	$14\frac{1}{4}$	$58\frac{1}{2}$
4	32	M	W	5	7	14
5	32	N	W	10	14	$14\frac{3}{4}$
6	32	N	W	$2\frac{1}{2}$	7	$28\frac{3}{4}$
7	8	M	H	5	7	$14\frac{3}{4}$
8	8	N	H	10	14	15
9	8	N	H	$2\frac{1}{2}$	7	$30\frac{1}{4}$
1	2	3	4	5	6	7

 The

The $51''\frac{1}{2}$ in number 3, column 7, was determined in fact from $29''\frac{1}{4}$, being the time of making 10 equable revolutions after the weight was dropped off, in order to prevent the sensible retardation that might take place, and affect the observation, if continued for 20 revolutions made so slowly.

FURTHER DEFINITIONS.

I have already defined what I mean by mechanic power; but, before I proceed further, it will be necessary also to define the following terms:

Impulse or Impulsion,
Impulsive Force or Power,
Impelling Force or Power, } By all which, I understand the uniform endeavour that one body exerts upon another, in order to make it move; and that, whether it produces or generates motion by this endeavour or not; and the quantity of this impelling power may be measured either by its being a weight of itself, or by being counterbalanced by a weight. It may also act either immediately upon the body to be moved, so that if motion is the consequence, they move with the same velocity; and that, either by a simple contact, or by being drawn as by a cord, or pushed as by a staff: or it may act by the intervention of a lever or other mechanic instrument, in which the velocity of the body to be moved may be very different from the velocity of the impelling power or mover; but in comparing them, the impelling powers must be reduced according to the proportional velocities of the mover and moved; or, in levers of different lengths, they may be compared by a standard length of lever, which is the method taken in the subsequent reasoning upon the preceding experiments. An impelling power, therefore, consisting of a double weight, or requiring a double weight to counterbalance it, when acting with equal levers, is a double impelling power, or an impelling power of double the intensity.

OBSER-

OBSERVATIONS AND DEDUCTIONS FROM THE PRECEDING EXPERIMENTS.

1ſt, By the firſt experiment it appears, that the mechanic power employed, conſiſting of 8 ounces in the ſcale, deliberately deſcending (by 5 turns of the bigger barrel) through a perpendicular ſpace $25\frac{1}{4}$ inches, will repreſent the quantity of mechanic power which cauſes the two heavy bodies, from a ſtate of reſt, to acquire a velocity, ſuch as to carry them equably through 20 circumferences of their circle of revolution in the ſpace of 29″; and that the time in which the mechanic power produced this effect was $14''\frac{1}{4}$, as appears by column 6th. And this mechanic power we ſhall expreſs by the number 202, the product of the number of ounces in the ſcale multiplied by the inches in its perpendicular deſcent, for $8 \times 25\frac{1}{4} = 202$.

2d, By the ſecond experiment, as 10 turns of the ſmaller barrel are equal to the ſame perpendicular height as 5 turns of the bigger, it follows, that the ſame mechanic power, *viz.* 202, acting upon the ſame heavy bodies to accelerate them, produces the very ſame effect in generating motion in the bodies as it did before, *viz.* 20 revolutions in $29''\frac{1}{4}$, the ſmall difference of $\frac{1}{4}$ of a ſecond being no more than may reaſonably be attributed to the unavoidable errors ariſing from friction of the machine, want of perfect accuracy in its meaſures, reſiſtance of the air, and imperfections in the obſervations themſelves, which muſt not only be allowed for in this, but the reſt; but as the impelling power is acting here upon a lever of but half the length, and, conſequently, but half the intenſity, when referred to the bodies to be moved, it takes juſt double the time to generate the ſame velocity therein.

DEDUCTION. It appears from hence, that the ſame mechanic power is capable of producing the ſame velocity in a given

body, whether it is applied ſo as to produce it in a greater or leſſer time; but that the time taken to produce a given velocity, by an uniformly continued action, is in a ſimple inverſe proportion of the intenſity of the impulſive power.

3dly, The third experiment being made with $2\frac{1}{2}$ turns of the leſſer barrel, the ſame weight in the ſcale of 8 ounces deſcending only 1-quarter part of the former perpendicular, the mechanic power employed will be only one quarter part of the former, *viz.* $50\frac{1}{2}$; but as one quarter part of the mechanic power produces half of the former velocity in the heavy bodies; that is, they make 20 revolutions in $58''\frac{1}{2}$ that is, nearly 10 revolutions in $29''$; we may conclude, in this inſtance, that the mechanic power, employed in producing motion, is as the ſquare of the velocity produced in the ſame body; and that the velocity produced is as the time that an impelling power, of the ſame intenſity, continues to act upon it, as appears by the near agreement of numbers 2 and 3, column 6th.

4thly, In the fourth experiment, the apparatus is the ſame as the firſt, only here the weight in the ſcale is 32 ounces; that is, the impelling power is the quadruple of the firſt, and hereby a double velocity is given to the bodies; for they make 20 revolutions in $14''$, which is a ſmall matter leſs than half the time taken up in making 20 revolutions in the firſt experiment. It alſo appears, that the velocity acquired is ſimply as the impelling power compounded with the time of its action; for a quadruple impulſion acting for $7''$ inſtead of $14''$ generates a double velocity, while the mechanic power employed to generate it is quadruple, for $32 \times 25\frac{1}{4} = 808$. And here the mechanic power employed being four times greater than the firſt, it holds here alſo, that the mechanic power, to be neceſſarily employed, is as the ſquare of the velocity to be generated; that is, in the ſame proportion as turned out in the third experiment, where the mechanic power employed was only a quarter part of the firſt.

5thly,

5thly, The fifth and fixth experiments were made with a mechanic power four times greater than thofe employed in numbers 2 and 3 refpectively; and fince the fame deductions refult from hence as from numbers 2 and 3, they are additional confirmations of the conclufions drawn from them and from the laft article.

6thly, In the feventh experiment, the difpofition of the apparatus is the fame as number 1, only here the bodies are placed upon the arms at the half-length; from whence it appears, that the fame mechanic power ftill produces the fame velocity in the fame bodies; for though 20 revolutions were performed in 14″$\frac{3}{4}$ (fee column 7) which is nearly half the time that 20 revolutions were performed in the firft experiment; yet, fince the circles in which the bodies revolved in the feventh are only of half the circumference as thofe of number 1, it is obvious, that the abfolute velocity acquired by the moving bodies in the two cafes is equal. But, by column 6th, the time in which it was generated is only half; yet, notwithftanding, this will coincide with the former conclufions, if the intenfity of the impelling power is compounded therewith; for, though the barrel was the fame with the fame number of turns as in number 1, and therefore the lever the fame, by which the impelling power acted, yet, as the bodies, upon which this lever was to act, were placed upon a lever of only half the length from the centre, the impelling power, acting by the firft lever, would act upon the fecond with double the intenfity, according to the known laws of mechanics; that is, it would require a double weight oppofing the bodies, to prevent their moving, in order to balance it. An impulfive power, therefore, of double the intenfity, acting for half the time, produces the fame effect in generating motion, as an impulfive power, of half the intenfity, acting for the whole time.

7thly, The eighth and ninth experiments afford the fame deductions and confirmations relative to the feventh experiment,

 that

that the fifth and sixth do refpecting the fourth, and that the fecond and third do refpecting the firft; and from the near agreement of the whole, when the neceffary allowances before mentioned are made, together with fome fmall inequality arifing from the mechanical power loft by the difference of the motion given by gravity to the weight in the fcale: I fay, from thefe agreements, under the very different mechanical powers applied, which were varied in the proportion of 1 to 16, we may fafely conclude, that this is the univerfal law of nature, refpecting the capacities of bodies in motion to produce mechanical effects, and the quantity of mechanic power neceffary to be employed to produce or generate different velocities (the bodies being fuppofed equal in their quantity of matter); that the mechanic powers to be expended are as the fquares of the velocities to be generated, and *vice verfâ*; and that the fimple velocities generated are as the impelling power compounded with, or multiplied by, the time of its action, and *vice verfâ*.

We fhall, perhaps, form a ftill clearer conception of the relation between velocities produced and the quantity of mechanic power required to produce them; together with the collateral circumftances attending, by which thefe propofitions, feemingly two, are reconciled and united, by ftating the following popular elucidation, which indeed was the original idea that occurred to me on confidering this fubject; to put which to an experimental proof gave birth to the foregoing apparatus and experiments.

Suppofe then a large iron ball of 10 feet diameter, turned truly fpherical, and fet upon an extended plane of the fame metal, and truly level. Now, if a man begins to pufh at it, he will find it very refifting to motion at firft; but, by continuing the impulfe, he will gradually get it into motion, and having nothing to refift it but the air, he will, by continuing his efforts, at length get it to roll almoft as faft as he can run.

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Suppose now, in the first minute he gets it rolled through a space of one yard; by this motion, proceeding from rest (similar to what happens to falling bodies) it would continue to roll forward at the rate of two yards *per* minute, without further help; but supposing him to continue his endeavours, at the end of another minute he will have given it a velocity capable of carrying it through a space of two yards more, in addition to the former, that is, at the rate of four yards *per* minute; and at the end of the third minute, he has again added an equal increase of velocity, and made it proceed at the rate of six yards *per* minute; and so on, increasing its velocity at the rate of two yards in every minute. The man, therefore, in the space of every minute exerts an equal impulse upon the ball, and generates an equal increase of movement correspondent to the definition of Sir Isaac Newton. But let us see what happens besides: the man, in the first minute, has moved but one yard from where he set out; but he must in the second minute move two yards more, in order to keep up with the ball; and as he exerted an impulse upon it, so as at the end of the second minute to have given it an additional velocity of the two yards, he must also in this time have gradually changed its velocity from the rate of two yards *per* minute to that of four, and the space, that he will of consequence have actually been obliged to go through in the second minute, will be according to the mean of the extremes of velocity at the beginning and end thereof, that is, three yards in the second minute; so that being one yard from his original place at the beginning of the second minute, at the end of it he will have moved the sum of the journies of the first and second minute, that is, in the whole four yards from his original place. As he has now generated a velocity in the ball of four yards *per* minute, in the third minute he must travel four yards to keep up with the ball, and one more in generating the equal increment of velocity; so that in the third minute, he must travel five yards to keep up the same impelling power

upon

upon the ball that he did in the firſt minute in travelling one, ſo that theſe five yards in the third minute added to the four yards that he had travelled in the two preceding minutes, ſets him at the end of the third minute nine yards from whence he ſet out, having then given the ball a velocity capable of carrying it uniformly forward at the rate of ſix yards *per* minute, as before ſtated. We may now leave the further purſuit of theſe proportions, and ſee how the account ſtands. He generated a velocity of two yards *per* minute in the firſt minute, the ſquare of which is four, when he had moved but one yard from his place; and he had generated a velocity of ſix yards *per* minute, the ſquare of which is thirty-ſix, at the end of the third minute, when he had travelled nine yards from his place. Now, ſince the ſquare of the velocity, generated at the end of the firſt minute, is to that of the velocity generated at the end of the third minute, as 4 : 36, that is, as 1 : 9; and ſince the ſpaces, moved through by the man to communicate theſe velocities, are alſo as 1 : 9, it follows, that the ſpaces through which the man muſt travel, in order to generate theſe velocities reſpectively (preſerving the impelling power perfectly equal), muſt be as the ſquares of the velocities that are communicated to the ball; for, if the man was to be brought back again to his original place by a mechanical power, equally exerted upon the man equally reſiſting, this would be the meaſure of what the man has done in order to give motion to the ball. It therefore directly follows, conformably to what has been deduced from the experiments, that the mechanic power that muſt of neceſſity be employed in giving different degrees of velocity to the ſame body, muſt be as the ſquare of that velocity; and if the converſe of this propoſition did not hold, *viz.* that if a body in motion, in being ſtopped, would not produce a mechanical effect equal or proportional to the ſquare of its velocity, or to the mechanical power employed in producing it, the effect would not correſpond with its producing cauſe.

Thus

Thus the consequences of generating motion upon a level plane exactly correspond with the generating of motion by gravity; *viz.* that though in two seconds of time the equal impulsive power of gravity gives twice the velocity to a body that it does in one second, yet this collateral circumstance attends it, that at the end of the double time, in consequence of the velocity acquired in the first half, the body has fallen from where it set forward through four times the perpendicular; and therefore, though the velocity is only doubled, yet four times the mechanical power has been consumed in producing it, as four times the mechanical power must be expended in bringing up the fallen body to its first place.

This then appears to be the foundation, not only of the disputes that have arisen, but of the mistakes that have been made, in the application of the different definitions of quantity of motion; that while those, that have adhered to the definition of Sir ISAAC NEWTON, have complained of their adversaries, in not considering the time in which effects are produced, they themselves have not always taken into the account the space that the impelling power is obliged to travel through, in producing the different degrees of velocity. It seems, therefore, that, without taking in the collateral circumstances both of time and space, the terms, quantity of motion, *momentum*, and force of bodies in motion, are absolutely indefinite; and that they cannot be so easily, distinctly, and fundamentally compared, as by having recourse to the common measure, *viz.* mechanic power.

From the whole of what has been investigated, it therefore appears, that time, properly speaking, has nothing to do with the production of mechanical effects, otherwise than as, by equally flowing, it becomes a common measure; so that, whatever mechanical effect is found to be produced in a given

time,

time, the uniform continuance of the action of the same mechanical power will, in a double time, produce two such effects, or twice that effect. A mechanical power, therefore, properly speaking, is measured by the whole of its mechanical effect produced, whether that effect is produced in a greater or a lesser time; thus, having treasured up 1000 tuns of water, which I can let out upon the overshot wheel of a mill, and descending through a perpendicular of 20 feet, this power applied to proper mechanic instruments, will produce a certain effect, that is, it will grind a certain quantity of corn; and that, at a certain rate of expending it, it will grind this corn in an hour. But suppose the mill equally adapted to produce a proportionable effect, by the application of a greater impulsive power as with a less; then, if I let out the water twice as fast upon the wheel, it will grind the corn twice as fast, and both the water will be expended and the corn ground in half an hour. Here the same mechanical effect is produced; *viz.* the grinding a given quantity of corn, by the same mechanical power, *viz.* 1000 tuns of water descending through a given perpendicular of 20 feet, and yet this effect is in one case produced in half the time of the other. What time, therefore, has to do in the business is this: let the rate of doing the business, or producing the effect, be what it will, if this rate is uniform, when I have found by experiment what is done in a given time, then, proceeding at the same rate, twice the effect will be produced in twice the time, on supposition that I have a supply of mechanic power to go on with. Thus 1000 tuns of water, descending through 20 feet of perpendicular, being, as has been shewn, a given mechanic power, let me expend it at what rate I will, if when this is expended, I must wait another hour before it be renewed, by the natural flow of a river or otherwise, I can then only expend twelve such quantities of power in 24 hours; but if, while I am expending 1000 tuns in one hour, the stream renews me the same quantity, then I can

I can expend 24 ſuch quantities of power in 24 hours; that is, I can go on continually at that rate, and the product or effect will be in proportion to time, which is the common meaſure; but the quantity of mechanic power ariſing from the flow of the two rivers, compared by taking an equal portion of time, is double in the one to the other, though each has a mill, that, when going, will grind an equal quantity of corn in an hour.

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NEW

FUNDAMENTAL EXPERIMENTS

UPON THE

COLLISON OF BODIES.

BY MR. JOHN SMEATON, F. R. S.

EXPERIMENTS

UPON THE

COLLISION OF BODIES.

Read before the Royal Society, April 18, 1782.

IT is univerſally acknowledged, that the firſt ſimple principles of ſcience cannot be too critically examined, in order to their being firmly eſtabliſhed; more eſpecially thoſe which relate to the practical and operative parts of mechanics, upon which much of the active buſineſs of mankind depends. A ſentiment of this kind occaſioned my tract upon *Mechanic Power* which is the ſecond paper of this volume. What I have now to offer was intended as a ſupplement thereto, and the experiments were then, in part, tried; but the completion thereof was deferred at that time, partly from want of leiſure; partly to avoid too great a length of the paper itſelf, and partly to avoid the bringing forward too many points at once.

My preſent purpoſe is to ſhew, that the true doctrine of the *colliſion of bodies* hangs as it were upon the ſame hook, as the doctrine of the gradual generation of motion from reſt, conſidered

dered in that paper: that is, that whether bodies are put into gradual motion, and uniformly accelerated from rest to any given velocity; or are put in motion, in an instantaneous manner, when bodies of any kind strike one another; the motion, or sum of the motions produced, has the same relation to mechanic power therein defined, which is necessary to produce the motion desired. To prove this, and at the same time to shew some capital mistakes in principle, which have been *assumed* as indisputable truths by men of great learning, is the reason of my now pursuing the same subject.

I do not mean to point out the particular mistakes which have been made by particular men, as that would lead me into too great a length: I shall therefore content myself with obser-ving, that the laws of collision, which have been investigated by mathematical philosophers, are principally of three kinds; *viz.* those relating to bodies perfectly *elastic*; to bodies perfectly unelastic, and perfectly *soft*; and to bodies perfectly unelastic, and perfectly *hard*. To avoid prolixity, I shall consider in each, only the simple case of two bodies which are equal in weight or quantity of matter striking one another. Respecting those which are perfectly elastic, it is universally agreed, that when two such bodies strike one another, no motion is lost; but that in all cases, what is lost by one is acquired by the other: and hence, that if an elastic body in motion strikes another at rest, upon the stroke the former will be reduced to a state of rest, and the latter will fly off with an equal velocity.

In like manner, if a non-elastic *soft* body strikes another at rest, they neither of them remain at rest, but proceed together from the point of collision with exactly one half the velocity that the first had before the stroke; this is also universally allow-ed to be true, and is fully proved by every good experiment upon the subject.

Respecting the third species of body, that is, those that are non-elastic and yet perfectly hard: the laws of motion relating to

to them, as laid down by one ſpecies of Philoſophers, have been rejected by another; the latter alledging, that there are no ſuch bodies to be found in nature whereon to try the experiment; but thoſe who have laid down and aſſigned the doctrine that would attend the colliſion of bodies, of this kind (if they could be found) have univerſally agreed, that if a non-elaſtic *hard* body was to ſtrike another of the ſame kind at reſt, that, in the ſame manner as is agreed concerning non-elaſtic ſoft bodies, they neither of them would remain at reſt, but would in like manner proceed from the point of colliſion, with exactly one half of the velocity that the firſt had before the ſtroke: in ſhort, they lay it down as a rule attending all non-elaſtic bodies, whether hard or ſoft, that the velocity after the ſtroke will be the ſame in both, *viz. one half* of the velocity of the original ſtriking body.

Here is therefore the aſſumption of a principle, which in reality is proved by no experiment, nor by any fair deduction of reaſon that I know of, *viz.* that the velocity of non-elaſtic *hard* bodies after the ſtroke muſt be the ſame as that reſulting from the ſtroke of non-elaſtic *ſoft* bodies; and the queſtion now is, whether it is true or not?

Here it may be very properly aſked, what ill effects can reſult to practical men, if philoſophers ſhould reaſon wrong concerning the effects of what does not exiſt in nature, ſince the practical men can have no ſuch materials to work upon, or miſjudge of? But it is anſwered, that they who infer an equality of effects between the two ſorts, may from thence be miſled themſelves, and in conſequence miſlead practical men in their reaſonings and concluſions concerning the ſort with which they have abundant concern, to wit, the non-elaſtic *ſoft bodies*, of which water is one, which they have much to do with in their daily practice.

Previous to the trying my experiment on mills, I never had doubted the truth of the doctrine, that the ſame velocity reſulted

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from the ſtroke of both ſorts of non-elaſtic bodies; but the trial of thoſe experiments made me clearly ſee at leaſt the inconcluſiveneſs, if not the falſity of that doctrine: becauſe I found a reſult which I did not expect to have ariſen from either ſort; and for the which, when it appeared from experiment, I could ſee a ſubſtantial reaſon why it ſhould take place in one ſort, and that it was impoſſible that it could take place in the other; for if it did, the bodies could not have been perfectly *hard*, which would be contrary to the hypotheſis: of this deduction I have given notice in my ſaid tract on mills, page 30. *The effect therefore of overſhot wheels, &c.*

It may alſo be ſaid, that ſince we have no bodies perfectly elaſtic, or perfectly unelaſtic and *ſoft*, why ſhould we expect bodies perfectly unelaſtic and *hard?* Why may not the effects be ſuch as ſhould reſult from a ſuppoſition of their being *imperfectly elaſtic* joined with their being *imperfectly hard?* But here I muſt obſerve, that the ſuppoſition appears to be a contradiction in terms.

We have bodies which are ſo nearly perfectly elaſtic, that the laws may be very well deduced and confirmed by them; and the ſame obtains with reſpect to non-elaſtic *ſoft* bodies; but concerning bodies of a mixed nature, which are by far the greateſt number, ſo far as they are wanting in elaſticity, they *are ſoft* and *bruiſe*, *yield* or *leave a mark* in colliſion; and ſo far as they are not perfectly ſoft they are elaſtic, and obſerve a mixture of the law relative to each; but imperfectly elaſtic bodies, imperfectly hard, come in reality under the *ſame deſcription* as the former mixed bodies: for ſo far as they are imperfectly hard they are ſoft, and either *bruiſe* and *yield*, or leave a mark in the ſtroke; and ſo far as they want perfect elaſticity, they are non-elaſtic; that is to ſay, they are bodies imperfectly elaſtic, and imperfectly ſoft; and in fact I have never yet ſeen any bodies but what come under this deſcription. It ſeems, therefore, that reſpecting the *hardneſs* of bodies they differ in degrees

grees of it, in proportion as they have a greater degree of tenacity or cohesion; that is, are further removed from perfect softness, at the same time that their elastic springs, so far as they reach, are very stiff; and hence we may (by the way) conclude, that the same mechanic power that is required to change the figure in a *small degree* of those bodies that have the popular appellation of *hard bodies*, would change it in a *great degree* in those bodies that approach towards softness, by having a small degree of tenacity or cohesion. In the former kind we may rank the harder kinds of *cast iron*, and in the latter *soft tempered clay*.

While the philosophical world was divided by the dispute about the *old* and *new opinion*, as it was called, concerning the powers of bodies in motion, in proportion to their different velocities: those who held the old opinion contending, that it was as the velocity *simply*, asked those of the new, How, upon their principles, they would get rid of the conclusions arising from the doctrine of unelastic perfectly hard bodies? They replied, they found no such bodies in nature, and therefore did not concern themselves about them. On the other hand, those of the new opinion asked those of the old, How they would account for the case of non-elastic soft bodies, where, according to them, the whole motion lost by the striking body was retained in the two after the stroke (the two bodies moving together with the half velocity,) though the two non-elastic bodies had been bruised and changed their figure by the stroke; for, if no motion was lost, the change of figure must be an effect without a cause? To obviate this, those of the old opinion seriously set about proving, that the bodies might change their figure, without any loss of motion in either of the striking bodies.

Neither of these answers have appeared to me satisfactory, especially since my mill experiments: for with respect to the first, it is no proper argument to urge the impossibility of finding

ing the proper material for an experiment, in anſwer to a concluſion drawn from an abſtract idea. On the other hand, if it can be ſhewn, that the figure of a body can be changed, without a *power*, then, by the ſame law, we might be able to make a *forge hammer* work upon a maſs of ſoft iron, without any other power than that neceſſary to overcome the friction, reſiſtance, and original *vis inertiæ* of the parts of the machine to be put in motion: for, as no progreſſive motion is given the maſs of iron by the hammer (it being ſupported by the anvil), no power can be expended that way; and if none is loſt to the hammer from changing the figure of the iron, which is the only effect produced, then the whole power muſt reſide in the hammer, and it would jump back again to the place from which it fell, juſt in the ſame manner as if it fell upon a body perfectly elaſtic, upon which, if it did fall, the caſe would really happen: the power therefore, to work the hammer would be the ſame, whether it fell upon an elaſtic or non-elaſtic body; an idea ſo very contrary to all experience, and even apprehenſion, of both the philoſopher and vulgar artiſt, that I ſhall here leave it to its own condemnation.

As nothing, however, is ſo convincing to the mind as experiments obvious to the ſenſes, I was very deſirous of contriving an experiment in point, and as I ſaw no hopes of finding matter to make a *direct* experiment, I turned my mind towards an indirect one: ſo circumſcribed, however, as to prove inconteſtably, that the reſult of the ſtroke of two non-elaſtic perfectly hard bodies could not be the ſame as would reſult from the colliſion of two ſoft ones; that is, if it can be *bona fide* proved, that *one half* of the original power is loſt in the ſtroke of ſoft bodies by the change of figure (as was very ſtrongly ſuggeſted by the mill experiments); then ſince no ſuch loſs can happen in the colliſion of bodies perfectly hard, the reſult and conſequence of ſuch a ſtroke muſt be *different*.

The conſequence of a ſtroke of bodies perfectly hard, but void of elaſticity, muſt doubtleſs be different from that of bodies perfectly

perfectly *elastic*: for having no spring the body at rest could not be driven off with the velocity of the striking body, for that is the consequence of the action of the spring or elastic parts between them, as will be shewn in the result of the experiments; the striking body will therefore not be stopped, and as the motion it loses must be communicated to the other, from the equality of action and re-action, they will both proceed together, with an equal velocity, as in the case of non-elastic soft bodies: the question, therefore, that remains is, what that *velocity must be?* It must be greater than that of the non-elastic soft bodies, because there is no mechanical power lost in the stroke. It must be less than that of the striking body, because, if equal, instead of a *loss* of motion by the collision it will be doubled. If, therefore, non-elastic soft bodies lose half their motion, or mechanical power, by change of figure in collision, and yet proceed together with half the velocity, and the non-elastic hard bodies can lose *none* in any manner whatever; then, as they must move together, their velocity must be such as to preserve the equality of the mechanic power, *unimpaired*, after the stroke the same as it was before it.

For example, let the velocity of the striking body before the stroke be 20, and its mass or quantity of matter 8; then according to the rule deduced from the experiments in the Tract on *Mechanic Power* (see exp. third and fourth) that power will be expressed by $20 \times 20 = 400$, which $\times\ 8 = 3200$; and if half of it is lost in the stroke, in the case of non-elastic soft bodies, it will be reduced to 1600; which $\div$ 16 the double quantity of matter will give 100 for the square of their velocity; the square root of which being 10, will be the velocity of the two non-elastic soft bodies after the stroke, being just one half of the original velocity, as it is constantly found to be. But in the non-elastic hard bodies, no power being lost in the stroke, the mechanic power will remain after it as before it, $= 3200$; this, in like manner, being divided by 16, the double quantity of matter, will give 200 for the square of the velocity, the square

 root

root of which is 14.14, &c. for their velocity after the ſtroke, which is to 10, the velocity of the non-elaſtic ſoft bodies after the ſtroke, as the ſquare root of 2 to 1, or as the diagonal of a ſquare to its ſide.

It remains, therefore, now to be proved, that preciſely half of the mechanic power is *loſt* in the colliſion of no-elaſtic ſoft bodies; for which purpoſe my mind ſuggeſted the following reflections. In the colliſion of elaſtic bodies the effect ſeemingly inſtantaneous, is yet performed in *time*; during which time the natural ſprings reſiding in elaſtic bodies, and which conſtitute them ſuch, are bent or forced, till the motion of the ſtriking body is divided between itſelf and the body at reſt; and in this ſtate the two bodies would then proceed together, as in the caſe of non-elaſtic ſoft bodies; but as the ſprings will immediately reſtore themſelves in an equal time, and with the ſame degree of *impulſive force*, wherewith they were bent in this re-action, the motion that remained in the ſtriking body will be totally deſtroyed, and the total exertion of the two ſprings, communicated to the original reſting body, will cauſe it to fly off with the ſame velocity wherewith it was ſtruck.

Upon this idea, if we could conſtruct a couple of bodies in ſuch a way that they ſhould either act as bodies perfectly elaſtic; or that their ſprings ſhould at pleaſure be hooked up, retained, or prevented from reſtoring themſelves, when at their extreme degree of bending; and if the bodies under theſe circumſtances obſerved the laws of colliſion of non-elaſtic ſoft bodies, then it would be proved, that one half of the mechanical power, reſiding in the ſtriking body, would be loſt in the action of colliſion; becauſe the impulſive force or power of the ſpring in its reſtitution being cut off, or ſuſpended from acting, which is equal to the impulſive force or power to bend it (and which alone has been employed to communicate motion from one body to the other) it would make it evident, that one half of the impulſive force is loſt in the action, as the other half remains *locked up* in the

the ſprings, it alſo follows, as a *collateral circumſtance*, that be the impulſive power of the ſprings what it may from firſt to laſt, yet as one half of the *time* of the action is by this means cut off, in this ſenſe alſo it will follow, that one half of the mechanic power is deſtroyed; or rather, in this caſe, remains locked up in the ſprings, capable of being *re-exerted* whenever they are ſet at liberty, and of producing a freſh mechanical effect, equivalent to the motion or mechanical power of the two non-elaſtic ſoft bodies after their colliſion.

Hence we muſt infer, that the quantity of mechanical power expended in diſplacing the parts of non-elaſtic ſoft bodies in colliſion, is exactly the ſame as that expended in bending the ſprings of perfectly elaſtic bodies; but the difference in the ultimate effect is, that in the non-elaſtic ſoft bodies, the power taken to diſplace the parts will be totally loſt and deſtroyed, as it would require an equal mechanic power to be raiſed a-freſh, and exerted in a contrary direction to reſtore the parts back again to their former places; whereas, in the caſe of the elaſtic bodies, the operation of half the mechanic power is, as obſerved already, only locked up and ſuſpended, and capable of being re-exerted without a further original acceſſion.

Theſe ideas aroſe from the reſult of the experiments tried upon the machine deſcribed in my ſaid tract upon mechanic power, and were alſo communicated to my very worthy and ingenious friend WILLIAM RUSSEL, Eſq. F. R. S. at the ſame time that I ſhewed him thoſe experiments in 1759; but the mode of putting this matter to a full and fair mechanical trial has ſince occurred; and though ſome rough trials, ſufficient to ſhew the effect, were made thereon, prior to the offering the Paper on Mechanical Power to the Society in 1776, yet the machine itſelf I had not leiſure to complete to my ſatisfaction till lately; which I mention to apologize for the length of time that theſe ſpeculations have taken in bringing forward.

DESCRIPTION OF THE MACHINE FOR COLLISION.

FIG. I. ſhews the front of the Machine as it appears at reſt when fitted for uſe.

A is the pedeſtal, and A B the pillar, which ſupports the whole, C D are two compound bodies of about a pound weight each, but as nearly equal in weight as may be. Theſe bodies are alike in conſtruction, which will be more particularly explained by fig. 2. Theſe bodies are ſuſpended by two white fir rods of about half an inch diameter, *e f* and *g h* being about four feet long from the point of ſuſpenſion to the centre of the bodies; and their ſuſpenſion is upon the croſs piece I I, which is mortiſed through, to let the rods paſs with perfect freedom; and they hang upon two ſmall plates filed to an edge on the under ſide, and paſs through the upper part of the rods. Their centers are at *k* and *l*, and the edges being let into a little notch, on each ſide the mortiſe, the rods are at liberty to vibrate freely upon their reſpective points (or rather edges) of ſuſpenſion, and are determined to one plain of vibration. M N is a flat arch of white wood, which may be covered with paper, that the marks thereupon may be more conſpicuous.

The croſs piece I I is made to project ſo far before the pillar, that the bodies in their vibrations may paſs clear of it, without danger of ſtriking it; and alſo the arch M N is brought ſo far forward as to leave no more than a clearance, ſufficient for the rods to vibrate freely without touching it.

FIG. 2. ſhews one of the compound bodies, drawn of its full ſize. A B is a block of wood, and about as much in breadth as it is repreſented in height, through a hole in which the wood rod C C paſſes, and is fixed therein.

DB

D B reprefents a plate of lead about three-eights of an inch thick, one on each fide, fcrewed on by way of giving it a competent weight. *d* B *e f g* reprefents the edge of a fpringing plate of brafs, rendered elaftic by hard hammering; it is about five eights of an inch in breadth, and about one twentieth of an inch thick. It is fixed down upon the wooden block at its end *d* B by means of a bridge plate, whofe end is fhewn *h i*, and is fcrewed down on each fide of the fpring plate by fcrews, which being relaxed the fpring can be taken out at pleafure, and adjufted to its proper fituation. *k l* is a light thin flip of a plate, whofe under edge is cut into teeth like a fine faw or ratchet, and is attached to the fpring by a pin at *k*, which paffes through it, and alfo through a fmall ftud rivetted into the back part of the fpring, and upon which pin, as a center, it is freely moveable. *m n* fhews a fmall plate or ftud feen edgeways raifed upon the bridge plate, through an hole in which ftud the ratchet paffes; and the lower part of the hole is cut to a tooth fhaped properly to catch the teeth of the ratchet, and retain it together with the fpring at any degree to which it may be fuddenly bent; and for this intent it is kept bearing gently downward, by means of a wire-fpring *o p q*, which is in reality double, the bearing part at *o* being femi-circular; from which branching off on each fide the rod C C, paffes to *p*, and fixes at each end into the wood at *q*. However to clear the ratchet, which is neceffarily in the middle as well as the rod, the latter is perforated; and alfo the block is cut away, fo far as to fet the mainfpring at *e* free of all obftacles that would prevent its play from the point B. The part *f g* is fhewn thicker than the reft, by being covered with thin kid leather tight fewed on, to prevent a certain jarring that otherwife takes place on the meeting of the fprings in collifion.

Let us now return to *fig.* 1. the marks upon the arch M N are put on as follows. *o p* is an arch of a circle from the centre *l*, and *q r* an arch of a circle from the centre *k*, interfecting each other at S. Now the middle line of the marks *t*, *v*, are at the fame diftance from the middle line at S that the centres *k l*

are;

are; ſo that when each body hangs in its own free poſition, without bearing againſt the other, the rod *e f* will cover the mark at *t*; and the rod *g h* will cover the mark at *v*. From the point S upon the arches S *p* and S *q* reſpectively, ſet off points at an equal and competent diſtance from S each way, which will give the middle of the mark *w* and *x*: and upon the arch S *p* find the middle point between the mark *v* and *w*, which let be *y*; and on the other ſide, in like manner, upon the arch S *q* find a middle point for the mark *z*; then ſet off the diſtance S *v* or S *t* from *y* each way, and from *z* each way; and from theſe points, drawing lines to the reſpective centres *l* and *k*, they will give the place and poſition of the marks *a*, *b*, and *c*, *d*; and thus is the machine prepared for uſe.

FOR TRIALS ON ELASTIC BODIES.

FOR this uſe take out the pins and ratchets from each reſpectively, and the ſprings being then at liberty, with a ſhort bit of ſtick (ſuppoſe the ſame ſize as the rods) turn aſide the rod *g h* with the right hand, carrying the body D upwards till the ſtick is upon the mark *w*, as ſuppoſe at o; there hold it, and with the left ſet the body C perfectly at reſt; in which caſe the rod *e f* will be over the mark *t*; then ſuddenly withdraw the ſtick, in the direction that the rod *g h* is to follow it, and the ſpring of the body D, impinging upon that of the body C, they will be both bent, and alſo reſtored; and the body C will fly off, and mount till its rod *e f* covers the mark *x*; the rod of the ſtriking body D remaining at reſt upon its proper mark of reſt *v*, till the body C returns, when the body D will fly off in the ſame manner; the two bodies thus rebounding a number of times, loſing a part of their vibration each time; but ſo nearly is the theory of elaſtic bodies fulfilled hereby, that the ſingle advantage of originally puſhing the rod *g h* beyond the mark *w*, by the thickneſs of the ſtick, or its own thickneſs, is ſufficient to carry the rod of the quieſcent body C completely to its mark *x*.

There

There are ſeveral other experiments which may be made with this apparatus, in confirmation of the doctrine of the colliſion of elaſtic bodies; which being univerſally agreed upon, and well known, it is needleſs further to dwell upon here; but reſpecting the application to non-elaſtic ſoft bodies, it is far more difficult to come at a fitneſs of materials for this kind of experiment, than it is for thoſe ſuppoſing perfectly elaſticity. The concluſions, however, may be attained with equal certainty.

FOR TRIALS ON NON-ELASTIC SOFT BODIES.

FOR this purpoſe the ratchets muſt be applied and put in order as before deſcribed, and the ſprings being both put to their point of reſt, let the body D be put to its mark *w* in the ſame manner as before deſcribed, and the body C to reſt. The body D being let go, and ſtriking the body C at reſt, in conſequence of the ſtroke, the ſprings being hooked up by the ratchets, they both move from their reſting marks *t*, *v*, reſpectly towards M: Now if they both moved together, and the rod *e f* covered the mark *c*, and the rod *g h* covered the mark *d* at their utmoſt limit, then they would truly obey the laws of non-elaſtic ſoft bodies; becauſe their medium aſcent would be to the mark *z*, which is juſt half the angle of aſcent to the mark *x*; but as in this piece of machinery, though the main or principal ſprings are hooked up, yet every part of them, and all the materials of which they are compoſed, and to which they are attached, have a degree, or more properly ſpeaking, a certain compaſs of *elaſticity*, which, as ſuch, is perfect, and no motion loſt thereby.

We muſt not, therefore, expect the two compound bodies after the ſtroke to ſtick together without ſeparating, as would be the caſe with bodies truly non-elaſtic and ſoft; but that from the elaſticity they are poſſeſſed of, they will by rebounding be ſeparated; but that elaſticity being perfect, can occaſion no loſs of motion to the ſum of the two bodies; ſo that if the body C aſcends as much above its mark *c* as the body D falls ſhort of its mark *d*, then it

it will follow, that their medium aſcent will ſtill be to the mark *z*, as it ought to have been, had they been truly non-elaſtic ſoft bodies; and this, in reality, is truly the caſe in the experiment, as nearly as it can be diſcerned.

After a few vibrations, by the rubbing of the ſprings againſt one another, they are ſoon brought to reſt; and here they would *always reſt* had they been truly and properly perfect non-elaſtic ſoft bodies; but here, as in the caſe of theſe bodies, by a change of the figure and ſituation of the component parts, there is expended one half of the mechanical power of the firſt mover, yet in this caſe the other half is not *loſt* but *ſuſpended* ready to be re-exerted whenever it is ſet at liberty; and that it is really and *bona fida one half*, and neither more or leſs appears from this uncontroverted ſimple principle, that the power of reſtitution of a perfect ſpring is exactly equal to the power that bends it. And this may, in a certain degree, be ſhewn to be fact by experiment, if there were any need of ſuch a proof; for if, when the bodies are at reſt after the laſt experiment the two rods are laſhed together at the bottom with a bit of thread, and then the ratchets unpinned and removed; on cutting the thread with a pair of ſciſſars they will each of them rebound, C towards M and D towards N, and if they rebounded reſpectively to *z* and *y*, the mechanical power exerted would be the ſame as it was after the ſtroke, when the mean of their two aſcents was up to the mark Z; but here it is not to be expected, becauſe not only the motion loſt by the friction of the ratchets is to be deducted, becauſe it had the effect of real non-elaſticity; but alſo the elaſticity that ſeparated them in the ſtroke, which was loſt in the vibrations that ſucceeded; neither of which hindered the mean aſcent to be to *z*; but yet, under all theſe diſadvantages in the machine (if not unreaſonably ill made) the rod *e f* will aſcend to *d*, and *g h* to *a*: and hence I infer, as a poſitive truth, that in the colliſion of non-elaſtic ſoft bodies, *one half of the mechanic power reſiding in the ſtriking body is loſt in the ſtroke.*

Reſpecting

Respecting bodies unelastic and perfectly hard, we must infer, that since we are unavoidably led to a conclusion concerning them, which contradicts what is esteemed a truth capable of the strictest demonstration; viz. that the velocity of the centre of gravity of no system of bodies can be changed by any collision betwixt one another, something must be assumed that involves a contradiction. This perfectly holds, according to all the established rules, both of perfectly elastic and perfectly non-elastic *soft* bodies; rules which must fail in the perfectly non-elastic *hard* bodies, if their velocity after the stroke is to the velocity of the striking body as one is to the square root of 2; for then the centre of gravity of the two bodies will by the stroke acquire a velocity greater than the centre of gravity the two bodies had before the stroke in that proportion, which is proved thus.

At the outset of the striking body, the centre of gravity of the two bodies in our case will be exactly in the middle between the two; and when they meet it will have moved from their half distance to their point of contact, so the velocity of the centre of gravity before the bodies meet will be exactly one half of the velocity of the striking body; and, therefore, if the velocity of the striking body is 2, the velocity of the centre of gravity of both will be 1. After the stroke, as both bodies are supposed to move in contact, the velocity of the centre of gravity will be the same as that of the bodies; and as their velocity is proved to be the square root of 2, the velocity of their centre of gravity will be increased from 1. to the square root of 2; that is, from 1. to 1.414, &c.

The fair inference from these contradictory conclusions therefore is, that an unelastic hard body (perfectly so) is a repugnant idea, and contains in itself a contradiction; for to make it agree with the fair conclusions that may be drawn on each side, from clear premises, we shall be obliged to define its properties thus: That in the stroke of unelastic hard bodies they can not *possibly* *lose*

lose any mechanic power in the ſtroke; becauſe no other impreſſion is made than the communication of motion; and yet they *muſt loſe a quantity* of mechanic power in the ſtroke; becauſe, if they do not, their common centre of gravity, as above ſhewn, will acquire an *increaſe* of velocity by their ſtroke upon each other.

In a like manner the idea of a *perpetual motion* perhaps, at firſt ſight, may not appear to involve a contradiction in terms; but we ſhall be obliged to confeſs that it does, when, on examining its requiſites for execution, we find we ſhall want bodies having the following properties; that when they are made to *aſcend* againſt gravitation their abſolute weight ſhall be *leſs*; and when they *deſcend* by gravitation (through an equal ſpace) their abſolute weight ſhall be greater; which, according to all we know of nature, is a *repugnant* or *contradictory idea*.

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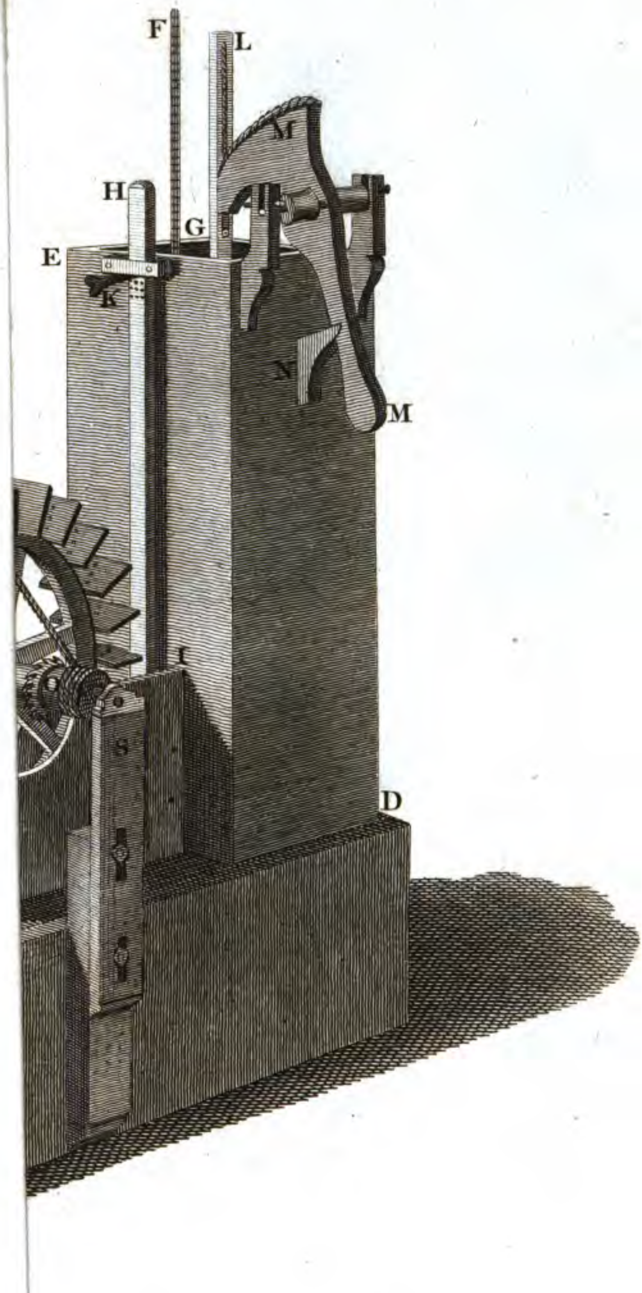

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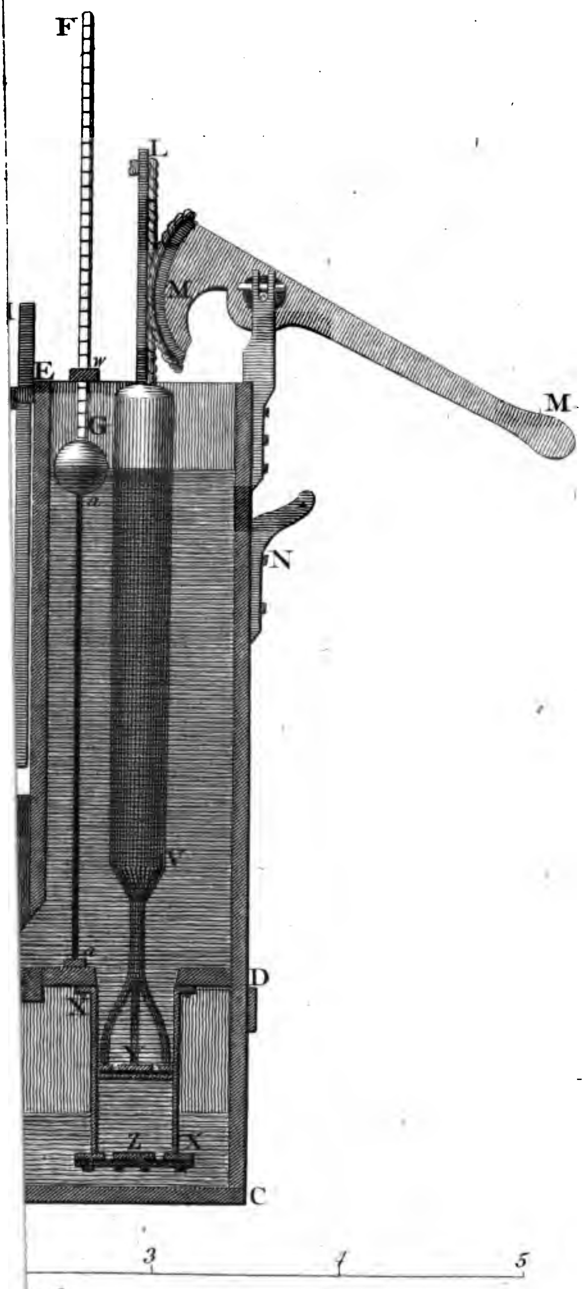

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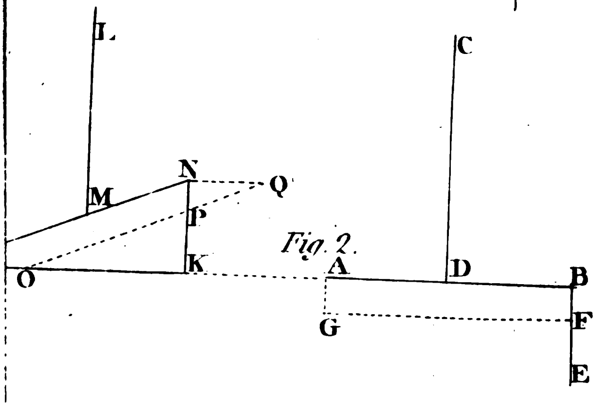

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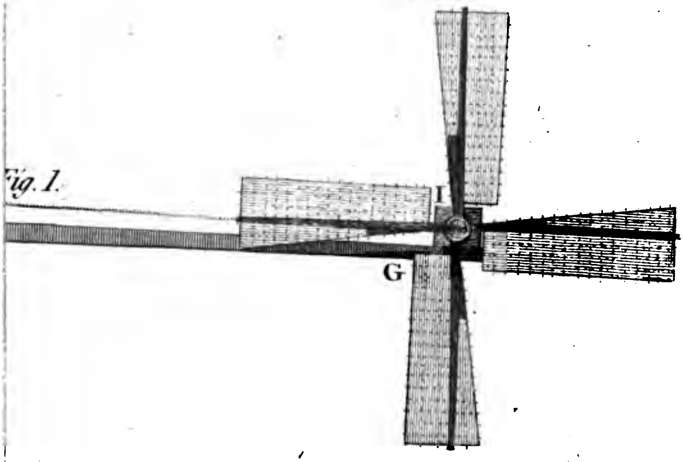

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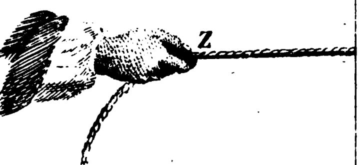

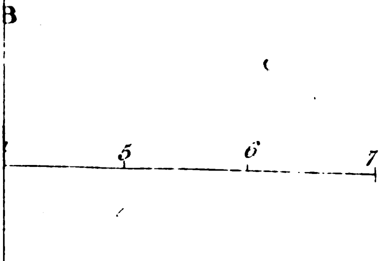

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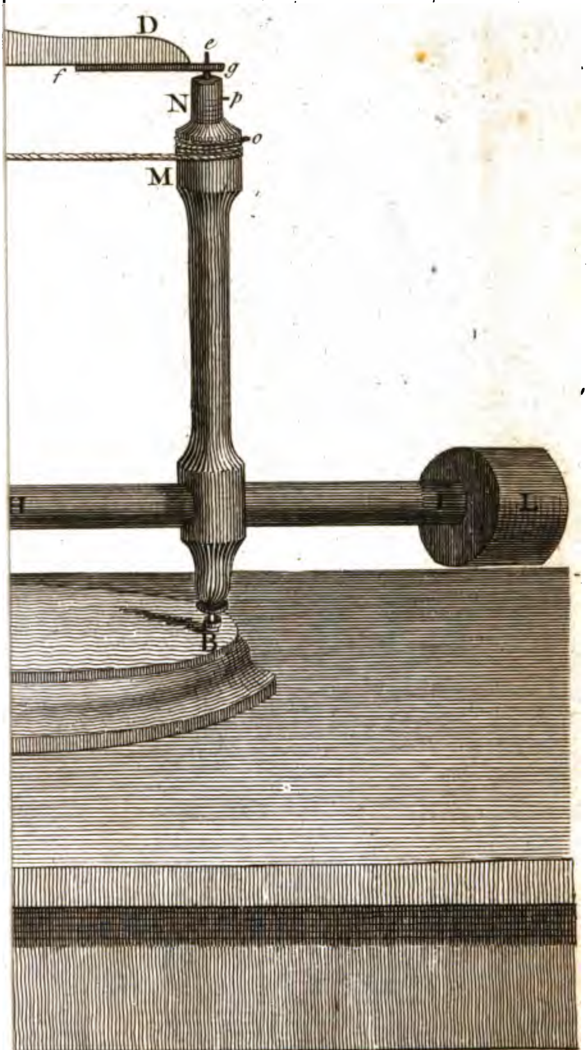

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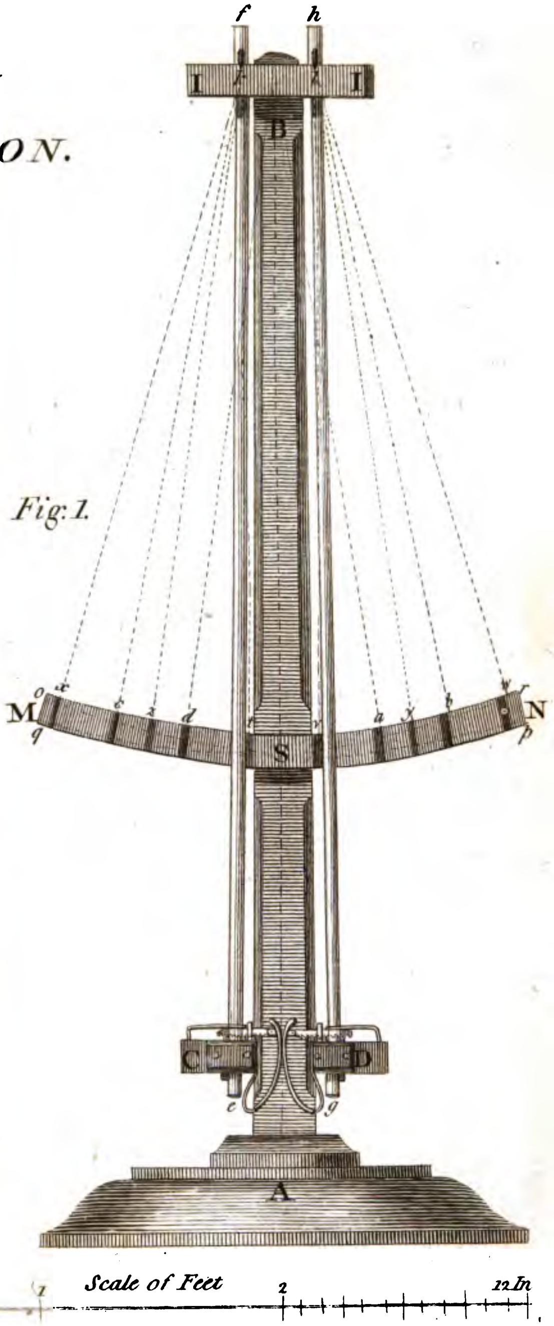

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